

THE MATHEMATICAL GAZETTE

EDITED BY
W. J. GREENSTREET, M.A.
WITH THE CO-OPERATION OF
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AND
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LONDON
G. BELL & SONS, LTD., PORTUGAL STREET, KINGSWAY, W.C. 2.
AND BOMBAY

Vol. XIII., No. 183. JULY, 1926. 3s. Net.

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VOL. XIII.

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No. 183.

COMPARISON BETWEEN RHUMB-LINE AND
GREAT-CIRCLE COURSES.*

BY PROF. P. J. HEAWOOD, M.A.

It is well known that, while the shortest course between two points on a sphere is a great circle, the course which is easiest for navigation is a rhumb line, i.e. a course which cuts all the successive meridians at the same angle, so that the ship always steers in a direction making an angle of the same number of degrees with due north or due south. Such a course is represented by a straight line on a Mercator's chart, which thus makes it appear more direct than it really is. On a great circle, though the course is the shortest, you are (except on the equator or a meridian) continually altering your direction as measured by the angle which it makes with the north. Between any two points there is thus a difference in favour of great-circle sailing; but to demonstrate rigorously the greatest value which this difference can possibly take is a somewhat difficult and attractive mathematical problem. I have heard it stated that it is not susceptible of rigorous mathematical treatment. The following is suggested as giving a complete solution of the question: it would be interesting to learn whether any shorter or more direct method is available, which makes no unwarranted assumption. For the abstract treatment of the problem (though indeed in the Pacific or the Southern Ocean fairly extended courses are practically available) we suppose the globe cleared of all such inconvenient encumbrances as continents or islands: moreover, we treat it as a perfect sphere; and to word the question more exactly we should say: Find two points such that the difference between the *shortest* great-circle course and the *shortest* rhumb-line course joining them is a maximum; for the great circle through two points will (in general) consist of a major and a minor arc, and on the other hand a rhumb line might start the longer way round the earth, or indeed make any number of revolutions about the pole before reaching its objective. In either case it is the shortest course of the kind with which we deal.

* The substance of this paper was read to the Mathematical Section of the University of Durham Philosophical Society on December 4th, 1925.

2. The first step is easy.

Suppose we start from a point P on the meridian OP , O being the pole, along a rhumb line PQQ' making an angle ϕ ($< \frac{\pi}{2}$) with OP and with each successive meridian (Fig. 1). Let the separate arcs PQ , PQ' be the great-circle courses from P to Q and Q' , QQ' again being joined by a great-circle arc. Then, in the spherical triangle PQQ' , $QQ' > PQ' - PQ$. *A fortiori* this will hold if for QQ' we substitute the portion of the rhumb-line course between them; i.e.

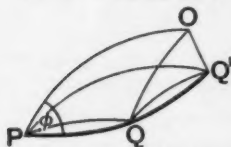


FIG. 1.

rhumb line PQ' - great circle $PQ' >$ rhumb line PQ - great circle PQ .

This shows that if we start along a given rhumb-line course the difference between that and the corresponding great-circle course becomes greater the further we go—until, that is, we reach a point whose longitude differs by 180° from that of the starting point. Beyond that we must not go; for to a further point on our course there would be a shorter rhumb-line course passing round the globe in the opposite direction. We conclude that at all events the greatest difference of courses will be between *some* two points which lie on opposite meridians.* The shortest great-circle course will then pass through one of the poles, which might be practically awkward; but it must be remembered that we are dealing with the abstract problem and have already abolished inconvenient continents and islands.

3. The next step requires some analysis. For a course extending through 180° of longitude we still have two quantities in our power— R_1 , the polar distance of the starting point, and ϕ , the angle at which the rhumb line cuts the meridians; on these R_2 , the final polar distance, depends. Let PQ (Fig. 2) be the complete rhumb-line course, subtending an angle π at pole O , $OP = R_1$, $OQ = R_2$ (O being chosen so that $R_1 + R_2 < \pi^\dagger$); Op ($=r$) a great-circle arc from O to any point on PQ , $\angle POp = \theta$, $Pp = \sigma$. By obvious infinitesimal geometry

$$\frac{dr}{d\sigma} = -\cos \phi; \dots\dots (1)$$

$$\frac{dr}{\sin r d\theta} = -\cot \phi. \dots\dots (2)$$

It follows that $R_1 - r = \sigma \cos \phi$, by (1);

$$\theta \cot \phi = - \int_{R_1}^r \frac{dr}{\sin r}, \text{ by (2),} \quad = \log \tan \frac{R_1}{2} - \log \tan \frac{r}{2},$$

giving a sort of polar equation to the rhumb line.

For the complete course, if $2x$ = rhumb-line length and $2z$ = great-circle length = $R_1 + R_2$,

$$R_1 - R_2 = 2x \cos \phi, \text{ by (1);}$$

and by (2)

$$\frac{\tan \frac{R_1}{2}}{\tan \frac{R_2}{2}} = e^{\pi \cot \phi}.$$

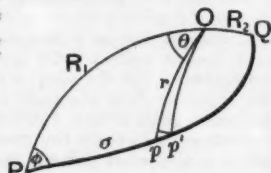


FIG. 2.

* It might be thought that the great circle joining two points on opposite meridians would be itself the limit of a rhumb line; but that is not so. You first go, say, due north, then due south. A rhumb line making a very small angle, ϕ , with meridians will follow a meridian at first very closely, but ends by circling sharply round the pole.

† This will always be possible, with $\phi < \frac{\pi}{2}$, for any course taken in one direction or the other.

in the equations above $\mu > 1 > \mu c$. Also these values give a true maximum for y . Further, as ϕ changes from 90° to zero, z and v continually diminish from 90° to $\cos^{-1} \frac{2}{\pi} (=50^\circ.46)$ and from 90° to 0, respectively; so that R_1 , which $= z + v$, ranges from 180° to $50^\circ.46$. Finally, for any ϕ , $R_1 = z + v$, $R_2 = z - v$ determine the extreme points of the course, while $x = v/c$.

By way of illustration, suppose $\phi = \frac{\pi}{4}$; e.g. let the rhumb-line course be one in which the vessel always steers north-west. The above results show that the difference of courses is a maximum when $z = 64^\circ.24$, $v = 55^\circ.69$; and so $R_1 = 119^\circ.93$, $R_2 = 8^\circ.55$; i.e. we start in latitude $29^\circ.93$ south and end in latitude $81^\circ.45$ north, with a difference of 180° in longitude, as shown by HK^* in Fig. 3, where the dotted line, just outside the hemisphere, shows the corresponding great-circle course. The lengths of the courses in great-circle degrees work out to 157.52 and 128.48, with a difference of 29.04—the maximum difference possible for this direction of the rhumb line.

5. The final question remains: for what value of ϕ will the maximum thus determined be greatest? This is the crux of the whole matter.

y being the difference, $y = x - z = v \sec \phi - z$,

where
$$\cos z = \frac{1}{t} \operatorname{cosec} h \frac{\pi}{2t}, \quad \cos v = \frac{1}{s} \sec h \frac{\pi}{2t}.$$

Hence
$$-\sin z \frac{dz}{d\phi} = -\frac{1}{s^2} \operatorname{cosec} h \frac{\pi}{2t} + \frac{1}{t} \frac{\cos h \frac{\pi}{2t}}{\sin h^2 \frac{\pi}{2t}} \times \frac{\pi}{2s^2}; \dots\dots\dots(1)$$

$$-\sin v \frac{dv}{d\phi} = -\frac{c}{s^2} \sec h \frac{\pi}{2t} + \frac{1}{s} \frac{\sin h \frac{\pi}{2t}}{\cos h^2 \frac{\pi}{2t}} \times \frac{\pi}{2s^2}. \dots\dots\dots(2)$$

Since
$$\frac{dy}{d\phi} = \frac{dv}{d\phi} \sec \phi - \frac{dz}{d\phi} + \frac{s}{c^2},$$

we deduce, by taking $(1) \times \tan h \frac{\pi}{2t} - (2) \times \sec \phi$, and simplifying (remembering that, as shown in § 3, $\sin z = \cot h \frac{\pi}{2t} \sin v$):

$$\frac{dy}{d\phi} = \frac{s \sin v}{c^2} \left(\frac{v}{\sin v} - \frac{\pi}{2st} \operatorname{cosec} h \frac{\pi}{2t} \right).$$

The first factor being necessarily positive, everything depends on the behaviour of the two functions inside the bracket.

Now we have seen in § 4 that, as ϕ ranges from 0 to $\frac{\pi}{2}$, v continually diminishes from $\frac{\pi}{2}$ to zero. Therefore $\frac{v}{\sin v}$ continually diminishes from $\frac{\pi}{2}$ to 1.

Again, as $\phi \rightarrow 0$, $\frac{\pi}{2st} \operatorname{cosec} h \frac{\pi}{2t}$ tends to zero;

as $\phi \rightarrow \frac{\pi}{2}$, $\frac{\pi}{2st} \operatorname{cosec} h \frac{\pi}{2t}$ tends to unity.

It might be rashly thought that, while the first function is always greater than unity, the other would be always less than unity, which would at once settle

* HK should terminate a little North of the 80° parallel. The dotted line should continue through the pole to K .

the sign of $\frac{dy}{d\phi}$. Unfortunately, however, that is not so. Calling the second function $\frac{1}{u}$ and putting k for $\frac{\pi}{2t}$, it will be found that

$$\frac{d}{d\phi} \left(\frac{1}{u} \right) = \frac{\sin^2 \phi}{u^2 \cos \phi} \left\{ -k^2 \left(\frac{4}{\pi^2} - \frac{1}{3} \right) + k^4 \left(\frac{2}{3\pi^2} + \frac{1}{30} \right) + \dots \right\},$$

where all the coefficients after the first are positive. Therefore, between $\phi=0$ and $\phi=\frac{\pi}{2}$, there is one (though only one) turning value, the sign changing from +, when k is large, to -, when k is small. The function $\frac{1}{u}$, in fact, first increases from zero to a value slightly greater than unity, and then slowly diminishes to unity. The behaviour of both functions is shown by the two curves plotted in Fig. 4, where it will be seen that, as they approach

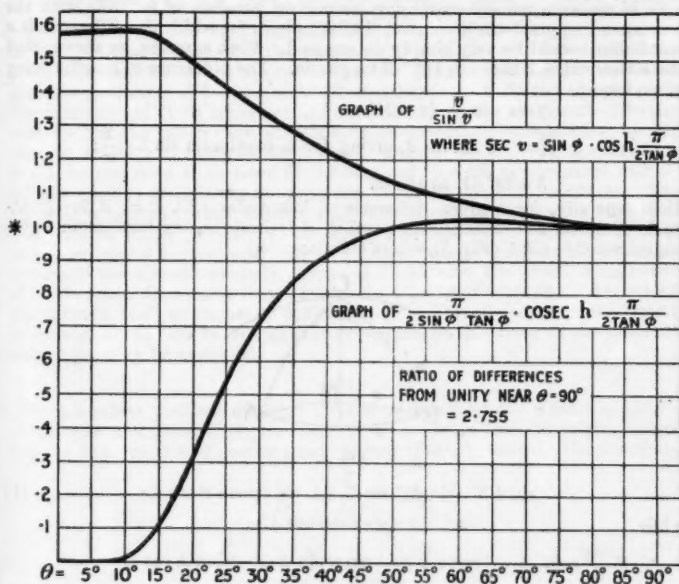


FIG. 4.

the limit, they become practically indistinguishable, and might conceivably intersect before the limit is reached. If, however, we expand in powers of k ($=\frac{\pi}{2t}$ as before), we find that when k is small,

$$\frac{v}{\sin v} = 1 + \frac{k^2}{6} \left(1 - \frac{4}{\pi^2} \right); \text{ while } \frac{\pi}{2st} \operatorname{cosec} h \frac{\pi}{2t} = 1 + \frac{k^2}{6} \left(\frac{12}{\pi^2} - 1 \right),$$

of which the former is the greater, since $1 - \frac{8}{\pi^2}$ is positive. In fact, the ratio of $\pi^2 - 4 : 12 - \pi^2$ is about 2.755; the ratio of the excesses above unity is therefore nearly 3 : 1. Thus the preponderance to the end of the first function

is assured, the differential coefficient is always positive, and y increases right up to $\phi = \frac{\pi}{2}$ (though, taking account of the factor outside the bracket, it will be found that the rate of change is zero, both for $\phi=0$ and $\phi=\frac{\pi}{2}$). Our conclusion is that the greatest possible difference of courses is when the rhumb line becomes merely a parallel of latitude extending through 180° of longitude—rather a commonplace conclusion, perhaps, to such a delicate investigation. Using the result of § 4, the polar distance of this parallel must $= \cos^{-1} \frac{2}{\pi} = 50^\circ.46$; i.e. latitude $= 39^\circ.54$. Such a course is indicated by XY (Fig. 3), the corresponding great-circle course passing through the pole. The problem proposed is thus completely solved, the lengths of the courses in great-circle degrees being by our formulae 138.81 and 100.92 respectively.

6. If we were merely comparing courses on parallels of latitude with the corresponding great-circle courses, the latitude λ , for which the difference is a maximum, would be very simply determined. First suppose, as above, that the course extends through 180° of longitude. The difference of lengths being y , we have :

$$y = \pi \cos \lambda - (\pi - 2\lambda);$$

$$\frac{dy}{d\lambda} = -\pi \sin \lambda + 2, \text{ giving for a maximum } \sin \lambda = \frac{2}{\pi};$$

$$\lambda = 39^\circ.54, \text{ as above.}$$

More generally, for a given difference of longitudes $= 2\theta$, $< \pi$; if $2x$ = great-circle distance, $2z$ = distance on parallel of latitude λ , we have from right-angled triangle CBN (Fig. 5), where C is the pole,

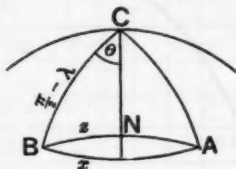


FIG. 5.

$$\sin BN = \sin BC \sin C, \text{ i.e. } \sin z = \sin \theta \cos \lambda; \dots\dots\dots(1)$$

while

$$x = y + z = \theta \cos \lambda.$$

$$\therefore \text{ if } \frac{dy}{d\lambda} = 0, \quad \cos z \frac{dz}{d\lambda} = -\sin \theta \sin \lambda = -\theta \sin \lambda \cos z,$$

giving

$$\cos z = \sin \theta / \theta. \dots\dots\dots(2)$$

Squaring and adding (1), (2), we have :

$$1 = \sin^2 \theta \cos^2 \lambda + \sin^2 \theta / \theta^2,$$

$$\text{i.e. } \cos^2 \lambda = \text{cosec}^2 \theta - 1/\theta^2, \quad \sin^2 \lambda = 1/\theta^2 - \cot^2 \theta.$$

$$\text{If } \theta = \frac{\pi}{2}, \quad \sin \lambda = \frac{2}{\pi}, \text{ as above.}$$

Suppose, however, $\theta = \frac{\pi}{4}$; $\sin^2 \lambda = \frac{16}{\pi^2} - 1$, $\lambda = 52^\circ$ very nearly. The courses are here shown by LM (Fig. 3), in the southern hemisphere. Their lengths in great-circle degrees are 55.41 and 51.61, with a difference of 3.8 only.

This illustrates how much less the difference is for shorter courses. But, further, it must be remembered that where the difference of longitudes of the extreme points is less than 180° , it is no longer true that *any* parallel of latitude will be the rhumb line yielding the greatest difference. An oblique course will, for a given difference of longitudes, make the difference greater. It will illustrate various aspects of the question if we take the rhumb line proceeding in a north-west direction from a point P on the equator and compare the distances PQ, PR, PS, PT, PU, PV (Fig. 3), measured on this line from P , with the corresponding great-circle courses, where the longitudes of Q, R, S, T, U, V differ from those of P by $30^\circ, 60^\circ, 90^\circ, 120^\circ, 150^\circ, 180^\circ$ respectively.*

We have (in great-circle degrees) :

Rhumb-line distances	40.61	72.59	94.06	107.42	115.49	120.28
Great-circle distances	40.58	71.80	90	96.97	97.22	94.95
Differences	-.03	-.79	4.06	10.45	18.27	25.33

We observe (1) how very small are the differences for the shorter courses, as compared with those for the longer; (2) that the great-circle distances positively diminish as we approach the 180° limit. Further, we may compare the maximum difference of 25.33, starting from P on the equator, with the difference of 29.04 which we before obtained as the maximum difference possible for the same direction—the north-westerly course shown by HK (Fig. 3), starting in latitude 29.93 south. Again, we see that the difference in the courses from P covering 90° of longitude is 4.06, as compared with 3.8 for the parallel of latitude 52° covering the same range of longitude. Thus an oblique course (taken haphazard) yields a greater difference than even that parallel which gives a maximum, for parallels, when the range of longitude is restricted in each case to 90° . Finally, we may compare the maximum difference for a north-westerly course of 29.04 with the absolute maximum of 37.89, where the rhumb-line course is the parallel of latitude $39^\circ.54$, extending through 180° , as shown at the end of § 5. For all the enormous variety of courses which may be brought into comparison, this value of the difference cannot possibly be exceeded.

II.

7. A kindred problem of equal interest is to determine when the *ratio* of rhumb-line and great-circle courses will be a maximum. It might, indeed, seem to be a question of greater practical importance to consider the percentage

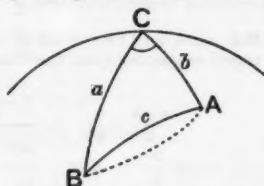


FIG. 6.

of gain in distance, due to great-circle sailing, than the absolute gain. There is also considerable mathematical interest in the contrast which this problem offers to the preceding. To begin with, what was there a simple first step—

* The courses are plotted, in the diagram, on a stereographic projection of the hemisphere. I find that in this case the projections of the rhumb lines which cut the meridians at the same angle ϕ will have points of inflexion all lying on that diameter of the bounding circle which is inclined at an angle ϕ , in the opposite sense, to the diameter through the poles. This property would make rather a pretty problem.

to show that, for an absolute maximum, the range of longitude must be 180° —here presents serious difficulties; we do better to begin by supposing *any* given difference of longitude between the initial and final points B and A (Fig. 6), C being the pole. In the spherical triangle CAB , the fundamental properties of the rhumb line investigated in § 3 give

$$\tan \frac{a}{2} / \tan \frac{b}{2} = e^C \cot \phi,$$

where ϕ is the constant angle ($< \frac{\pi}{2}$) at which the rhumb line (shown dotted in the figure) cuts the successive meridians.

Hence

$$\frac{\sin \frac{a-b}{2}}{\sin \frac{a+b}{2}} = \tan h \left(\frac{C \cot \phi}{2} \right),$$

and is therefore constant if C and ϕ are constant.

Starting with this supposition, we are led to a rather pretty piece of spherical trigonometry. We have:

$$\tan \frac{1}{2}(A-B) = \frac{\sin \frac{1}{2}(a-b)}{\sin \frac{1}{2}(a+b)} \cot \frac{C}{2}; \dots\dots\dots(1)$$

[One of Napier's Analogies.]

$$\sin \frac{1}{2}(A-B) = \frac{\sin \frac{1}{2}(a-b)}{\sin \frac{1}{2}c} \cos \frac{C}{2}. \dots\dots\dots(2)$$

[One of Delambre's Analogies.]

The first shows that with C given and $\sin \frac{1}{2}(a-b)/\sin \frac{1}{2}(a+b)$ given (as above), $A-B$ is constant. Then the second shows that $\sin \frac{1}{2}(a-b)/\sin \frac{1}{2}c$ is also constant.

Taking logarithms and differentiating, we have:

$$\frac{d(a-b)}{\tan \frac{a-b}{2}} = \frac{dc}{\tan \frac{c}{2}},$$

the variable element, with C and ϕ given, being the distance of the path from the pole.

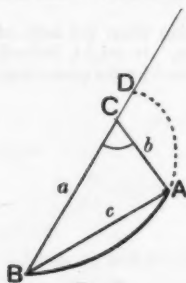


FIG. 7.

$$\text{Thus } \frac{d(a-b)}{a-b} \times \frac{\frac{a-b}{2}}{\tan \frac{a-b}{2}} = \frac{dc}{c} \times \frac{\frac{c}{2}}{\tan \frac{c}{2}}.$$

But since any two sides of a spherical triangle are together greater than the third,

$$\frac{a-b}{2} < \frac{c}{2}; \therefore \frac{\frac{a-b}{2}}{\tan \frac{a-b}{2}} > \frac{\frac{c}{2}}{\tan \frac{c}{2}}.$$

Hence

$$\frac{d(a-b)}{a-b} < \frac{dc}{c}.$$

Since c is the length of the great-circle course and $(a-b) \sec \phi$ the length of the rhumb-line course, the result, which gives

$$\frac{d\{(a-b) \sec \phi\}}{dc} < \frac{(a-b) \sec \phi}{c},$$

shows that the ratio of the courses thus measured is increased as their lengths are diminished, i.e. it is larger the nearer they approach the pole. Ultimately

the rhumb-line degenerates into an equiangular spiral, as indicated in Fig. 7, while the great-circle course becomes a straight line.

8. For the spiral we have :

$$\frac{(\text{great-circle course})^2}{(\text{rhumb-line course})^2} = \frac{a^2 - 2ab \cos C + b^2}{(a-b)^2 \sec^2 \phi} = \left(1 + \frac{4ab}{(a-b)^2} \sin^2 \frac{C}{2}\right) \cos^2 \phi.$$

But for the spiral, much as before for the rhumb line,

$$\frac{a+b}{a-b} = \cot h\left(\frac{C}{2} \cot \phi\right), \quad \text{and} \quad \therefore \frac{4ab}{(a-b)^2} = \operatorname{cosec} h^2\left(\frac{C}{2} \cot \phi\right);$$

therefore the ratio in question

$$\begin{aligned} &= \cos^2 \phi \left\{ 1 + \sin^2 \frac{C}{2} \div \sin h^2\left(\frac{C}{2} \cot \phi\right) \right\} \\ &= \cos^2 \phi + \sin^2 \phi \left\{ \frac{\left(\sin \frac{C}{2}\right)^2}{\frac{C}{2}} \div \left\{ \frac{\sin h\left(\frac{C}{2} \cot \phi\right)}{\frac{C}{2} \cot \phi} \right\}^2 \right\}. \end{aligned}$$

But, of the ratios in brackets, the first diminishes with C up to $C = \phi$, and the second increases, ϕ being constant. Therefore (since $\sin^2 \phi$ is multiplied by the first and divided by the second) the whole diminishes as C increases; and the above ratio is least, and so the ratio of rhumb-line course to great-circle course is greatest when $C = \pi$, as indicated (Fig. 7) by the spiral as completed by the dotted line.

9. Putting $C = \pi$, the ratio which is to be a maximum reduces to

$$(a-b) \sec \phi \div (a+b),$$

i.e.

$$\sec \phi \cdot \tan h\left(\frac{\pi \cot \phi}{2}\right) \equiv u, \text{ say.}$$

$$\therefore \frac{du}{d\phi} = \frac{\sin \phi}{\cos^2 \phi} \tan h\left(\frac{\pi}{2t}\right) - \sec \phi \cdot \sec h^2\left(\frac{\pi}{2t}\right) \times \frac{\pi}{2} \operatorname{cosec}^2 \phi$$

(using t as before for $\tan \phi$),

$$= \frac{\sin \phi}{\cos^2 \phi} \sec h^2 \frac{\pi}{2t} \left(\sin h \frac{\pi}{2t} \cos h \frac{\pi}{2t} - \frac{\pi}{2} \cot \phi \operatorname{cosec}^2 \phi \right).$$

Twice the quantity in brackets, on which the sign depends,

$$\begin{aligned} &= \sin h \frac{\pi}{t} - \frac{\pi}{t} \left(1 + \frac{1}{t^2}\right) \\ &= \left(\frac{\pi^3}{3} - \pi\right) \frac{1}{t^3} + \frac{\pi^5}{5} \frac{1}{t^5} + \dots, \end{aligned}$$

which is positive, as long as t is positive.

Therefore u increases with ϕ until $\phi = \frac{\pi}{2}$, when the spiral degenerates into a small semicircle about the pole as centre and the ratio of courses $= \frac{\pi}{2} = 1.5708$.

This is the absolute maximum possible, and though, pushed to this extreme point, the result is an absurdity, we may put our conclusion in the practical form that, for all possible courses, the ratio must be less than 1.571. Illustrations of possible ratios would be given by using the lengths calculated in Part I. For the courses whose difference is an absolute maximum (§ 5), the ratio is 1.375; where the difference is a maximum for the N.W. direction (§ 4), the ratio is 1.226.

January, 1926.

PERCY J. HEAWOOD.

SOME POINTS ON THE TEACHING OF RATIONAL MECHANICS.*

BY PROF. L. N. G. FILON, F.R.S.

1. *Meaning and importance of rational mechanics.*

By *rational mechanics* is meant the development of the subject of Mechanics according to a strict logical scheme, proceeding, like Geometry, from certain hypotheses or axioms established (or indicated) by experiment, from which particular theorems or applications are deduced. Every branch of theoretical physics aims at such a complete rational treatment, but whereas in Geometry the axioms have long ago been classified and isolated, and the bulk of the science consists of the deductions from them, in Physics we are still largely occupied with the discovery of the laws and axioms.

Newtonian Mechanics occupies a somewhat intermediate position: it is based on a certain set of hypotheses which, for a long time, have been unquestioningly accepted, and which form the basis of far-reaching and complicated deductions. The universal validity of these hypotheses has of late years, owing to the development of the Quantum and Relativity theories, been challenged, and it may well be that, after all, Newtonian Mechanics is only an approximate fit to the phenomena. Nevertheless, it forms a self-contained system; and the fact that it may not be the best representation of the totality of the data of observation is no more a reason for lack of logic in its presentation than the existence of non-Euclidean Geometries is an excuse for slipshod reasoning in an account of the properties of Euclidean space.

Moreover, the subject normally forms the first introduction of the student to the fundamental concepts of time, force, mass and energy, and to a systematic attempt at giving a rational account of the phenomena of Nature. Historically and educationally it is of great importance.

Unfortunately the subject suffers from the attitude of mind of the two classes of teachers who are usually entrusted with it: the mathematicians look upon it largely as a peg for hanging examples on algebra and differential calculus; the investigation of principles is frequently replaced by a few dogmatic statements, and, with an ill-concealed impatience of difficulties which he is unwilling to face, the teacher hurries on to a mass of numerical or algebraic examples, often of a highly artificial nature.

On the other hand, the physicists are largely preoccupied with modern electromagnetic theories of matter and give to mechanics a secondary place, being little anxious to develop it as an independent system. A number of formulae are obtained, often in a very sketchy and imperfect manner, assumptions being introduced whenever convenient, regardless of redundancy and logical order, and these formulae are then "verified" by a certain number of stock experiments, without any adequate consideration of what any one experiment does, or, what is often more important, what it does *not*, prove.

2. *Scope of the following discussion.*

A complete account of the logical difficulties introduced by the usual treatment of mechanics would be far beyond what can be dealt with in a short address. It is proposed in what follows to limit the discussion to a few of the more important points relating to the conceptions of velocity and acceleration and to Newton's laws of motion. These by no means exhaust even the hypotheses or axioms of ordinary mechanics; and the difficulties in the treatment of weight or gravitation are deliberately omitted, as taking us too far afield.

* A lecture delivered to the London Branch of the Mathematical Association, Feb. 26, 1926.

Nor is it in any way intended to lay down a scheme of teaching suitable for the *beginner*. Every teacher must find out from his own experience the particular process by which new ideas are best imparted; and it does not follow that this process need, or should, be logical. But it is of great importance that the teacher himself should have a clear conception of what is the true logical order, and of what are the real logical difficulties. With this knowledge he can so adjust his teaching that it shall, at any rate, not become a positive hindrance when his pupil is sufficiently advanced to begin the study of the subject on sound logical lines.

3. *Absolute direction and time.*

At the basis of rational Newtonian Mechanics are two fundamental assumptions. The first is that we can always determine when two *directions* in space are *identical*. The practical test (every definition must, of course, rest on a practical test) is when the two directions point to the same star. This assumes that the stars are so far away that two lines to the same star are always parallel. We know that this is not strictly the case for the nearer stars, nor, theoretically, is it accurate for *any* star, but it is still a fundamental assumption that, on the average, and restricting our attention to the faint stars of the celestial background, we obtain by this method a test of identity of direction. In any case, no other test is known.

The second is that we are able to form a consistent judgment (it may be by indirect reasoning) of simultaneity, and to say without error that two events, however widely separated in space, are truly simultaneous. This really implies the possibility of instantaneous signalling, which we now believe to be physically unattainable. But it is not impossible to think of; and it was the normal way of thinking about time until certain minor phenomena led to the theory of relativity.

If this assumption about simultaneity be made, it follows that, by taking any convenient variable quantity in the universe, *e.g.* the angle turned through by the Earth, we can construct an absolute scale of time which shall be valid throughout the universe.

Modern theories of space and time deny entirely both these assumptions: we must therefore, at this stage, part from their company; but the modifications they bring in are negligible in ordinary terrestrial mechanics, and comparatively slight even in celestial mechanics.

In what follows these two assumptions are implied throughout.

4. *Position, velocity and acceleration.*

Although absolute direction and absolute time are a necessity of Newtonian Mechanics, the same is by no means true of *absolute position*, and here we come to the first of a group of fallacies which are firmly rooted in the teaching of the subject, namely those which are connected with the use of so-called "fixed" axes of reference. Nothing is more confusing to the learner than to be made to solve a problem in mechanics under gravity at the Earth's surface, in which the laboratory, or the ground, is treated as *fixed*, while he is told, on the other hand, that these so-called "fixed" axes are carried round by the rotation of the Earth and by the annual motion of the Earth about the Sun, with a possible motion of the Sun itself thrown in. A fog arises as to the validity of the whole process, which no *a posteriori* explanation suffices to dispel.

The first point to emphasise is that all position is defined with regard to some point of reference, which can be identified in (or defined with regard to) some material body. This point is the origin of axes of coordinates, and since (*ex hypothesi*) the directions of these can be specified absolutely, we shall, at this stage, assume them invariable, so that rotation of the axes will not come in. Thus, instead of speaking of a *frame* of reference, it will be sufficient to speak of an *origin* of reference *O*, and to define the position of *P* with regard

to O by the directed straight line or vector OP . But whatever method of presentation be adopted, position must always be relative, never absolute.

What is true of position is equally true of velocity and of acceleration. If, in the space of which O is the origin, we draw the radii vectores OP , which represent the successive positions of P with regard to O , we obtain a curve in this space: this is the path of P with regard to O . By considering now in the usual way the limiting value of the vector $PP'/(t' - t)$, where P, P' are positions at very near times t, t' , we arrive at the definition of the velocity of P relative to O .

If any subsidiary origin V is then taken, and a vector VQ is drawn to represent the velocity of P with regard to O at time t , Q describes a path known as the *hodograph* of the motion of P with regard to O . If we look upon velocity as the rate of change of position, and define acceleration as the rate of change of velocity, we have at once that the acceleration of P relative to O is the velocity of Q relative to V , the fundamental property of the hodograph.

"Velocity" and "acceleration," without specification of a point of reference, are entirely meaningless.

5. The parallelograms of velocities and accelerations.

Since a point clearly cannot be in two places at the same time, there can be only one position vector at a given time, with regard to a given origin. And, for the same reason that a one-valued function cannot have two differential coefficients, a point P can have, with regard to the same origin O , only *one* velocity and *one* acceleration.

This brings us at once up against the usual statement of the parallelogram of velocities, in which a point is said to have two (simultaneous) velocities (presumably with reference to the same origin, though this is, as a rule, omitted). The difficulties of many proofs of this theorem are well known, in particular that connected with the "ladder" diagram occurring in some text-books, in which a number of small *successive* displacements, in two directions alternately, merge by a curious process, neither instinctive nor logical, into a continuous displacement in a third direction. After this achievement the parallelogram of accelerations is not infrequently disposed of by the comforting, if hardly illuminating statement that the same clearly holds good of the velocities added per unit of time. The author underwent much mental suffering, as a student, from this line of argument.

The plain truth is, that the parallelograms of velocities and accelerations, if stated in the above form, are simply nonsense. You cannot combine two things which do not exist. And the matter is made much worse when we find, as an afterthought, a rule for finding *relative* velocities and accelerations brought in as a distinct theorem.

If a little commonsense thought, however, is given to the question, it will be found that every single *physical* example which can be given of the composition of velocities is an example of the addition of *relative* velocities (e.g. the case of a man walking across a railway carriage), and the theorem should be stated as follows: the velocity of A with regard to B = vector-sum of velocity of A with regard to C and velocity of C with regard to B . Of course, the moment we consider different origins of reference, the same point may have different velocities. The theorem may then be proved with perfect ease by means of a sheet of tracing paper, on which a point C is marked; and a point A travels from C on the sheet of tracing paper in a definite direction with speed v . While it does this the sheet of tracing paper is itself moved without rotation, the point C separating from the point B , which it originally covered, with speed u . Simultaneous positions of B, C, A are then the corners of a triangle of which $BC = ut$, $CA = vt$ and the velocity of A with regard to B is \vec{BA}/t .

The same treatment, applied to the motions in the hodographs, gives the parallelogram of relative accelerations.

6. The laws of motion.

We are now in a position to consider the real significance of Newton's celebrated laws of motion in this system of mechanics. Difficulties arise at once from the use of the word "body" in these laws, since we cannot speak of the velocity or acceleration of an *extended* body. This set of difficulties, however, belongs to a group which it is not proposed to discuss in this lecture; so we will begin by putting them definitely on one side and restricting the "body" to be so small that it can be treated geometrically as a point. In the usual nomenclature, it will be a "particle."

When the usual explanations have been given, *e.g.* that "change of motion" is to be interpreted as "mass-acceleration," there remain two difficulties in the Second and Third Laws (the First Law is in a class by itself and will be considered later) which are rarely dealt with adequately.

The first difficulty is that the laws speak of the velocity or the acceleration, without specifying the origin of reference. This is due to the fact that Newton himself seems to have assumed *absolute position*, although, as will be shown, this is not a logical necessity of his system. But since we have given up "fixed" axes, it is clear that the origin of reference must be specified, for, if this is left arbitrary, the velocity or acceleration may be any whatever.

The second difficulty relates to *mass*. Newton himself defined it as volume \times density: and said that density was a concept of which everyone had intuitive knowledge; but since density cannot be measured, except by mass of unit volume, or by bringing in the consideration of *weight*, which is, at this stage, irrelevant, this definition really defines nothing. "Quantity of matter" may clearly be dismissed as mere verbiage.

It thus looks, at first sight, as though the laws depended on an undefinable position and an equally undefinable mass, and thus were incapable of being tested, which, if true, would remove them from the list of physical laws.

7. The modification of the Second Law.

This, however, is not the case: the answer to the first difficulty is provided by Newton's own corollary to the Second Law, to wit, that every force produces independently its own acceleration.

What interpretation is to be put upon this statement? Imagine two similar non-interfering particles *P* and *Q* moving under precisely similar conditions, except that in one case a force *F* is acting, in the other case it is not acting. There may be any number of unknown forces (the same for *P* and for *Q*) acting on the particles, and the origin of reference *O* may be moving in any manner. Then by the parallelogram of accelerations as we have established it, the acceleration of *P* with regard to *O* = acceleration of *Q* with regard to *O*, together with the acceleration of *P* with regard to *Q*.

Thus, the acceleration of *P* with regard to *Q* is the additional acceleration produced by the force *F*. But this result is entirely independent of the position of *O* and requires no hypothesis about "fixed" axes.

How are we to meet the difficulty about mass? Clearly the only thing that we know of it at this stage is that it remains the same for the same body, being some coefficient of proportionality between force and acceleration.

The Second Law must therefore be restricted to give us a means of comparing forces applied to the *same* body, and should be restated as follows:

Forces acting on *any given body* are proportional in magnitude to the *additional* accelerations which they produce and are in the same direction.

It is important to notice that the law, in this form, is purely a definition, and can never be contradicted by experience, since, whatever the manner in

which a body moves, we may always (*e.g.* in the case of gravity) postulate an invisible force which causes it to move in the required manner.

The Second Law implies the result that forces on the same particle combine according to the parallelogram law. For if P, Q, R be the positions of three similar particles, at time t , the first with neither of forces F_1, F_2 acting, the second with F_1 acting alone, the third with both F_1 and F_2 acting; then we have: acceleration of R relative to P = vector-sum of acceleration of R relative to Q and acceleration of Q relative to P . Multiplying by the inertia-coefficient of proportionality, the mass-acceleration due to F_1 and F_2 together is the vector-sum of F_1 and F_2 : which is the single force equivalent to F_1 and F_2 . It will be noticed that this method of combining forces is valid only for forces acting on the same particle.

8. The significance of the Third Law.

If two forces F_1 and F_2 acting on different bodies produce accelerations f_1, f_2 , then

$$F_1 = m_1 f_1; \quad F_2 = m_2 f_2,$$

where m_1, m_2 are coefficients of proportionality belonging to each body. If, however, we have no test for comparing forces such as F_1 and F_2 , applied to different bodies, no further progress can be made, and we are left with a definition of little value.

The Third Law provides the required test of equality of forces acting on different bodies: it thus enables us to compare masses, and its verification establishes the validity of the concept of mass. It is therefore, in a sense, the true Physical Law, and the one which depends upon experiment.

If the bodies in question move under their mutual reactions,

$$F_1 = F_2 \quad \text{and} \quad m_1/m_2 = f_2/f_1.$$

This enables the inertia coefficients to be compared.

Two fundamental tests must be carried out to establish the Third Law. First we have to show that, for bodies 1 and 2, the ratio f_2/f_1 , which we may call r_{12} , is invariable, under all conditions of mutual reaction. Secondly, if we apply the tests with a third body 3, we must have

$$r_{12} = r_{13}/r_{23}.$$

This last ensures that a body has the same inertia coefficient, whatever body it reacts with. The body 3 may be assigned arbitrarily a coefficient unity. Then $r_{13} = m_1$, $r_{23} = m_2$ and $r_{12} = m_1/m_2$, that is $m_1 f_1 = m_2 f_2$, which proves the Third Law.

Thus mass appears as an inertia coefficient, of which the definition and measurement are contained in the laws themselves. To Ernst Mach we owe the recognition of the importance of the Third Law in this connection.

A student who has once grasped the significance of the above argument is never likely afterwards to make the common confusion between *mass* and *weight*.

9. The experimental verification of the Third Law.

The experimental verification of the Third Law, described in the last section, may easily be carried out by means of a modification of the well-known "Fletcher trolley" apparatus. This is shown diagrammatically in Fig. 1. Two wooden trolleys, mounted on smooth aluminium wheels, and whose masses can be adjusted by suitable loading, are placed on a carefully levelled board, in such a way that they can move backwards and forwards along the same track. Each carries a light projecting flat wooden board ($A_1 B_1$, $A_2 B_2$); these are placed at the same level, sufficiently high to clear the bodies of the trolleys, and out of the central line so as to just clear one another while moving

side by side as the trolleys travel. Attached to them are two strips of paper * arranged so as to overlap slightly during the motion, so that they actually slide lightly over one another. In the (fixed) line LM a pencil or inked brush * can oscillate rapidly, so as to make marks across both strips at rapid intervals. In practice it is convenient to use for this purpose an oscillating metal lath, but there is no necessity to do this; the marks may even be made at irregular intervals, and no time measurement, or investigation of the law of oscillation, is necessary. All that is essential is that the crossing mark should always be made at the same position LM .

The trolleys are connected by a light spiral spring or piece of elastic C_1C_2 , and cork buffers and pins (D_1, E_1 ; D_2, E_2) are provided, as a locking arrangement to ensure that they move together after impact.

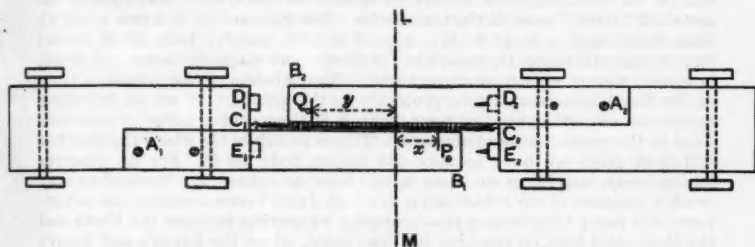


FIG. 1.

To carry out the experiment, the trolleys are separated, so that the spring or elastic is stretched; and they are held apart by a string passing round an arrangement of pulleys, not shown in the figure.

The recording pencil or brush is then set in motion, and makes a trace (many times repeated) on both papers, P_0, Q_0 being the crossing points. While the oscillations of the brush continue the trolleys are released by burning the string and move towards one another.

Then at any instant the distances x, y of P_0, Q_0 from LM give simultaneous displacements of the trolleys from rest. From the successive crossing points of the tracing brush on the edges of the paper strips we obtain a set of simultaneous values $(x_1, y_1), (x_2, y_2)$, etc. When the trolleys come together they stop dead. This is usually a most striking feature of the experiment. The tracing brush then inks in a final mark, giving the total distances travelled.

If the values y_1, y_2, \dots are then plotted on squared paper to the corresponding values x_1, x_2, \dots , the points will be found to lie very closely on a straight line, so that we have

$$y = kx,$$

whence $\dot{y} = k\dot{x}$, so that \ddot{x} and \ddot{y} both vanish when $\dot{x} + \dot{y} = 0$, i.e. when the trolleys come together.

Also $\ddot{y} = k\ddot{x}$, i.e. the accelerations are in a fixed proportion; and this proportion is independent of the nature of the connection between the trolleys. The slope k of the diagram then gives the constant r_{12} previously mentioned.

The experiment is repeated, using three different loadings of the trolleys in pairs, and the law

$$r_{12} = r_{13}/r_{23}$$

is verified. In this way the Second (modified) and Third Laws of motion are verified and the validity of the concepts of force and mass established.

* Omitted in the figure, not to overload the diagram.

10. *The First Law of Motion.*

We come now to the First Law, that "a body remains in a state of rest or of uniform motion in a straight line, unless it is compelled by force to change that state." This law is quite independent of the Second and Third Laws, provided the Second Law is restated as has been shown; but this fact is not usually appreciated.

Now at first sight it is clear that this law must definitely require a special kind of origin of reference: otherwise we could vary the motion without varying the forces. It thus appears that the law really defines certain preferential origins, or systems of axes of reference, which make it true. The question immediately arises: do such systems exist? We could only answer the question by finding a body which we are *certain* is acted on by *no force* at all. If we could find such a body we should, in effect, have materialised the so-called "fixed" axes of the text-books. But how are we to know when we have found such a body? We may, it is true, remove from it all *contact* forces: this still leaves the possibility of electric and magnetic forces. Assume, however, we are able to remove these. Nevertheless, if we release a body at the Earth's surface, it falls promptly to the ground. If we are unwilling to assume any other kind of force, then it is clear that a frame of reference fixed in the ground is unsatisfactory. To get an origin for which the first law will hold good, we have to take the falling body itself. For an observer falling freely, weight, as we know it, will have no existence. Some of us may recall a romance of our school days, in which Jules Verne describes the adventures of a party of travellers in a projectile wandering between the Earth and the Moon, and how, on reaching the dead point, where the Earth's and Moon's attractions balance, all the objects in the shell, including the passengers, begin to float about freely. As a matter of fact this curious state of affairs need not have waited to appear until that peculiar point had been reached, but should have occurred so soon as the projectile was free of the resistance of the Earth's atmosphere. To the passengers of such a shell, the first law of motion would be an obvious thing, and no need would arise for introducing any mysterious forces. In the same way the inhabitants of the Moon could not be conscious of the Earth's attraction, nor could we ever become dynamically aware of any accelerations possessed by ourselves and the Earth as a whole.

The difficulty arises when the passengers become aware of another shell, moving freely over their antipodes, and discover that it has, with regard to them, an acceleration $2g$. They are therefore driven (unless they are disciples of Einstein) either to throw overboard the First Law of Motion, or to postulate some invisible force to account for the relative motion. As we anticipate, they then find it convenient to transfer their origin to the centre of the Earth and to suppose that both shells move under a force directed thereto. The details, however, need not concern us at present. The essential point is that they are compelled to postulate the existence of some invisible force to cause the observed motion. Such a force we may refer to as a "gravitational" force.

But once the possibility of assuming such "gravitational" invisible forces is admitted, the problem presented by Newton's First Law takes on a totally different aspect. For now it is clear that all origins are equally acceptable and that no preference need be given to any of them, save for reasons of mathematical simplicity and convenience. Whatever our origin, we can still save Newton's First Law, provided we assume a gravitational force which will give a free particle the correct accelerations. Obviously some axes will lead to far more complicated gravitational forces than others. An observer who took his origin at a point fixed in a ship at sea would have to deal with gravitational forces which would be very awkward functions of space and time. But, whatever the motion of the ship, he could, if he so wished, suppose by this device that Newton's First Law held good for these axes.

In practice, then, we may regard Newton's First Law merely as defining the gravitational forces belonging to our particular origin of reference. If we take our origin at the surface of the Earth, then the "centrifugal force" of the Earth's rotation has to be included in so-called gravity, and is, in fact, so included. If our origin is at the centre of the Earth, this "centrifugal force" need no longer be postulated. If we further shift our origin to the centre of the Sun, an important part of the solar "tide generating force" (that which is accounted for by the acceleration of the Earth itself) disappears. *Provided we include the correct forces*, any origin and set of axes (fixed in direction) will verify the first law.

Now this is precisely the property which belongs to the "fixed" axes of the text-books. It follows that *every* origin may be regarded as a "fixed" origin. This alone is sufficient to show the absurdity of the term: a far better name, which would convey the real meaning, would be *Newtonian* origin (or axes)—Einstein uses the term "Galilean" frame of reference.

But the determination of the particular gravitational forces which have in each case to be assumed becomes a matter for experiment and observation, so that the Laws of Motion really do little more than establish the concept of mass and leave us at the threshold of Newtonian Mechanics.

L. N. G. FILON.

GLEANINGS FAR AND NEAR.

369. The Stirling Jug.—"The person whom we have to thank for this good service was the Reverend Alexander Bryce, minister of Kirkcubright, near Edinburgh, a man of scientific and literary accomplishments, much superior to what was displayed by the generality of the clergy of his day. Mr. Bryce, who had taught the mathematical class in the College of Edinburgh during the winter of 1745-6 instead of the eminent MacLaurin (who was then on his death-bed), happened to visit Stirling in the year 1750, etc., etc."—Extract from *Memoir of R. Chambers*, by his brother W. Chambers (p. 181).

370. The following is an extract from a letter in the *Radio Times* of 13th March, 1925:

"It is amusing for a listener to write about a composer whom he frankly admits he does not understand. On his own admission, he is yoked to a partner who does understand Bach, and surely he would have been better advised to have made a study of the composer before writing what may be intended to be humorous, but which, nevertheless, is ridiculous.

"I suppose 'E. L. B.' once went to school, and there, like most of us, had to study Euclid. He would meet the problem, 'Prove the half equal to the whole,' and, along with the rest of us, would exclaim: 'Absurd! how can the half equal the whole?' However, as Euclid was part of the curriculum, he would set about to prove the problem. The argument he would go through in this problem would strengthen his powers of reasoning and understanding. Similarly, if he would take the trouble to understand the monumental compositions of Bach, he would again have to admit the same result."—Per Prof. H. T. H. Piaggio.

371. It is a position in the *Mathematiques* that there is no proportion between somewhat and nothing, therefore the degree of nullitie and quidditie or act, seemeth larger then the degrees of increase and decrease. . . .—*The Coulters of Good and Evil*, § 10, Bacon.

372. Under the "Material Errata" prefaced to *Practical Education*, by Maria and R. L. Edgeworth, vol. i. 1798, we find:

P. 508, l. 17. For *arithmetick* read *prudence*.

A PROBLEM IN FOURFOLD GEOMETRY.

By D. B. MAIR, M.A.

We take two bodies P Q which have straight world lines OP KQ . This means in classical language that P and Q have a constant relative velocity. Our object is to find the configuration which an observer U reckons that of nearest approach of P and Q , and to find U 's measure of the least distance.

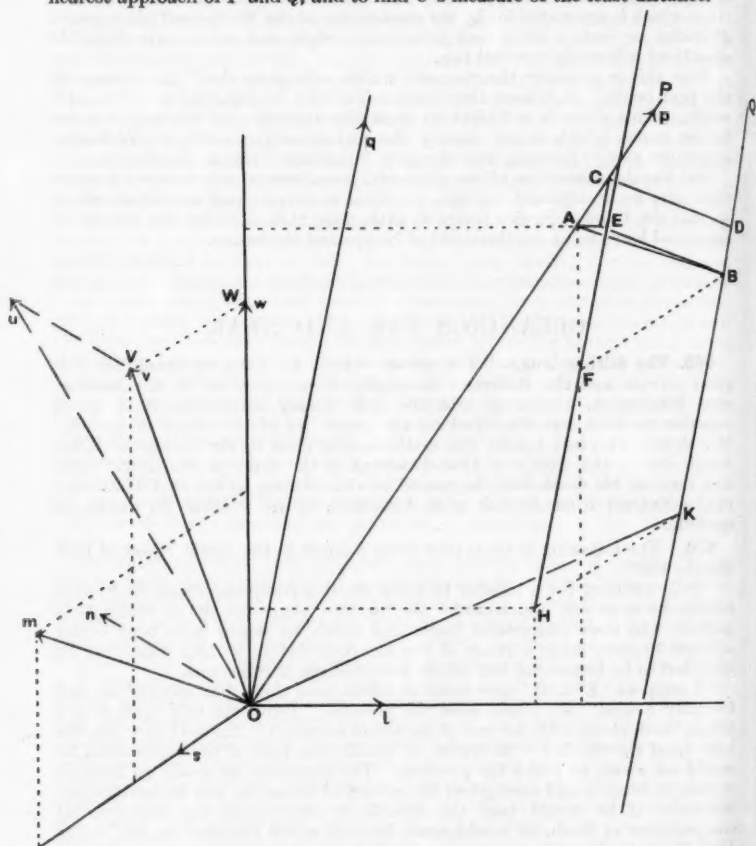


FIG. 1.

As origin we take O on P 's world line. We lay vector \mathbf{p} along OP , we denote vector \vec{OK} by \mathbf{k} , and we take at the origin a vector \mathbf{q} parallel to KQ . The three vectors \mathbf{p} \mathbf{q} \mathbf{k} determine a triplane $\mathbf{p}\mathbf{q}\mathbf{k}$; we denote the vector normal to this triplane by \mathbf{n} .

We shall discuss the position of nearest approach as reckoned by an observer U whose world line is \mathbf{u} or OU . U 's space at his zero of time is the triplane

through O normal to u . This triplane cuts the biplane pq in a line OL , along which we lay vector l .

The three vectors n u l determine a triplane nul ; the normal to this triplane we denote by m .

U 's space at any time is a triplane normal to u . The positions of P Q for U at that time are the intersections A B of the world lines OP KQ by this triplane, and AB is his reckoning of the distance between them at that time.

Let U 's space a moment later cut the world lines in C D . We complete the parallelogram $BDCE$. Then AE lies in the biplane pq , since AC and CE do so. And it is normal to u , since AB and BE are so. These two conditions fix the direction of AE ; and l was fixed by the same conditions; therefore AE is parallel to l .

Now suppose AB to be the least distance between P Q as reckoned by U . Then the infinitesimal displacement to CD leaves the distance unaltered, so that BA and BE are equal. That is to say, BA is normal to AE and to l .

To fix the direction of AB we now know that it lies in the triplane pqk or is normal to n , that it is normal to u , and that it is normal to l . Now m was taken normal to n u l , so that AB is parallel to m .

Let us draw the figure (Fig. 1). We begin with the biplane pq , P 's biplane. It contains w the projection on pq of U 's world line u , and perpendicular to w the line l in which U 's space at his time zero cuts the biplane. Behind pq is the parallel biplane containing KQ , Q 's biplane. The separation between P 's and Q 's biplanes, taken perpendicular to them, is HK , which is parallel to s the vector that lies in the triplane pqk and perpendicular to pq .

v being the projection of u on the triplane pqk , w the projection of u on pq is the projection of v parallel to s ; so that v lies in the biplane ws . As m is perpendicular to n and l it lies in ws ; and as m is perpendicular to u it is also perpendicular to v the projection of u on pqk .

BF being drawn parallel to s to meet pq in F , the triangle ABF has FA and FB at right angles. The angle between BA and BF is a hyperbolic angle θ , and BF or HK is equal to $AB \cosh \theta$. This angle θ is equal to the angle between m and s , and also (since m and v are at right angles) to the angle between v and w .

Consider first an observer U whose world line u lies in the biplane pq , so that u v coincide with w . In this case the angle θ is zero, and AB is equal to HK . In the view of this observer P Q are travelling in the same

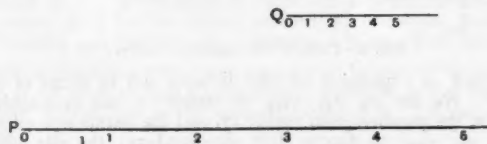


FIG. 2.—Paths as they appear to observer W .

direction, P along vector l and Q parallel to it. Fig. 2 shows the paths with simultaneous positions marked.

Consider now an observer whose world line u lies in the triplane pqk , so that u coincides with v . The distance AB is now less than HK , since $\cosh \theta$ is greater than unity. The greater the angle θ which u makes with the biplane pq , the less is U 's measure of AB ; and when θ is infinite, m and v coincide, and U 's measure is zero.

In the opinion of this observer the path of P is in the plane lm and the path of Q in a parallel plane. For both the component velocity

parallel to m is $\tanh \theta$, and when θ is infinite the component is unity and this observer's measure of the distance AB is zero. Fig. 3 shows the paths.

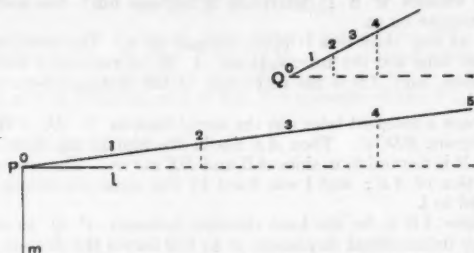


FIG. 3.—Paths as they appear to observer V .

Consider lastly the case in which u lies outside the triplane pqk . Everything depends on the position of v the projection of u on the triplane, and is included in the preceding discussion. We note that AB cannot be zero for our present observer, for even if the position u gives the velocity of light, the projection v gives a lesser velocity.

Relative to U the paths of PQ have components in the direction of l , in the direction of m , and in the direction normal to the plane lm . The two paths do not intersect.

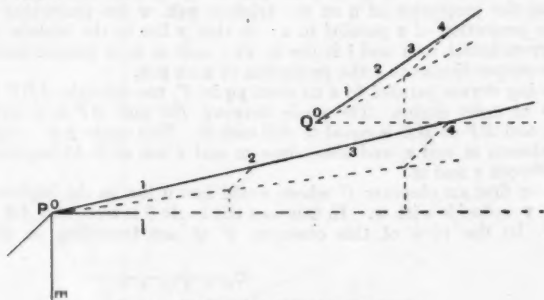


FIG. 4.—Paths as they appear to observer U .

We can find an expression for the distance AB in terms of the vectors $p q k v$. We use $\sigma_x \sigma_{xy} \sigma_{xyz}$ to denote the size or magnitude of the line vector x , the parallelogram vector xy , and the parallelepiped vector xyz ; we use $v_x v_{xy} v_{xyz}$ to denote their size-numbers; the size-number being the square of the size if the vector quantity has an even number of space-like dimensions, and the square taken negatively if the number of space-like dimensions is odd. The expressions for the size-numbers in terms of the vectors are ($x \cdot y$ denoting the inner product of x and y):

$$v_x = x \cdot x$$

$$v_{xy} = \begin{vmatrix} x \cdot x & x \cdot y \\ y \cdot x & y \cdot y \end{vmatrix}$$

$$v_{xyz} = \begin{vmatrix} x \cdot x & x \cdot y & x \cdot z \\ y \cdot x & y \cdot y & y \cdot z \\ z \cdot x & z \cdot y & z \cdot z \end{vmatrix}$$

The size σ_{pqv} of the parallelepiped pqv is the product of the base σ_{pq} by the height WV or $\sigma_v \sinh \theta$, so that

$$\sinh \theta = \sigma_{pqv} / \sigma_{pq} \sigma_v$$

$$\sinh^2 \theta = -\nu_{pqv} / \nu_{pq} \nu_v$$

$$\cosh^2 \theta = (\nu_{pq} \nu_v - \nu_{pqv}^2) / \nu_{pq}^2 \nu_v^2$$

The size σ_{pqk} is the product of the base σ_{pq} by the height HK , so that

$$HK = \sigma_{pqk} / \sigma_{pq}$$

$$HK^2 = -\nu_{pqk} / \nu_{pq}$$

And since $AB = HK / \cosh \theta$, we have

$$AB^2 = \frac{\nu_{pqk} \nu_v}{\nu_{pqv} - \nu_{pq} \nu_v}$$

D. B. MAIR.

373. Aristotle, conjured up by a Laputan, remarked that:—"... the vortices of Descartes were equally to be exploded. He predicted the same fate to attraction, whereof the present learned are such zealous asserters. He said that new systems of nature were but new fashions, which would vary in every age; and even those, who pretend to demonstrate them from mathematical principles, would flourish but a short period of time, and be out of vogue when that was determined."—*Gulliver's Travels*.

374. "My father was elected to a Subsizarship at Trinity in 1848. . . . For mathematics he had a considerable aptitude, but he always maintained humorously that his success in them was hampered by an indolent Coach, whose name I forget. My father used to give a most absurd account of his visits to this Coach, who was a pronounced Evangelical: he was always in bed when his pupils arrived, and was roused with difficulty. He used then—through a chink in the bedroom door—to propound a sum to fill up the time. Then he made an elaborate toilet, singing hymns all the time out of a little volume with immense unction, occasionally putting in a head accompanied by the hymn-book at the door and propounding another problem. My father said that the result of this species of coaching was that he went into the Trigonometry paper of the Mathematical Tripos ignorant of the meaning of the symbol π Eventually he came out a Senior Optime in Mathematics (in spite of π)."—*The Life of Edward White Benson, Sometime Archbishop of Canterbury*, by his Son, A. C. Benson, c. ii. (per P. J. Harris).

375. William Emerson (1701-1782) contributed largely to the mathematical periodicals of his time, though almost always under some fanciful name such as *Merones*, *Philofluentimecanalgegeomastrolongo*.—V. "Emerson," *Penny Cyclopaedia*.

376. *Circumference*. . . . We do not know why, but this word is always applied to a curvilinear figure; while the synonymous Greek word *periphery* is used for a rectilinear figure.—[? De Morgan], *Penny Cyclopaedia*.]

377. Sanity does not allow the infinitely little to disturb us.—Meredith, *One of Our Conquerors*, c. ii.

378. For a translation into Ido of Halley's Epitaph on Newton, v. *Notes and Queries*, 12 S. xi. 296, and for a free translation into English, v. *Martin's Gen. Mag. Arts and Sciences*, Jan. 1755.

379. "He's not a gentleman: he works!"—The Duke of Devonshire, of his famous nephew, Henry Cavendish, chemist, physicist, and millionaire.

MATHEMATICAL NOTES.

838. [K¹. 8. a.] *Note on Certain Poristic Cases of the Deformable Plane Quadrilateral.*

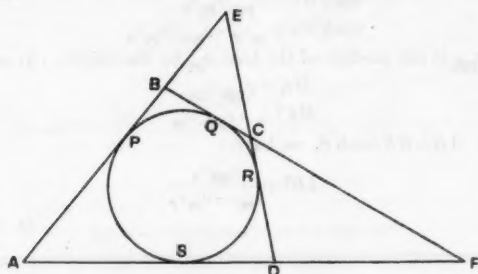


FIG. 1.

Considering Fig. 1, we have the well-known relation :

$$AB + CD = AD + BC.$$

We have also

$$EP + FQ = ER + FS,$$

that is,

$$AE - AP + FC + CQ = EC + CR + FA - AS,$$

or

$$AE + FC = EC + FA.$$

In addition,

$$EB + BP + FB - BQ = ED - DR + FD + DS,$$

or

$$EB + FB = ED + FD.$$

Considering Fig. 2, we have $AP = AS$, that is,

$$AB + BC + CR = AD + DC + CQ,$$

or

$$AB + BC = AD + DC.$$

Also, since $AP = AS$, we have

$$AE + EQ = AF + FR,$$

that is,

$$AE + EC - CQ = AF + FC - CR,$$

or

$$AE + EC = AF + FC.$$

Finally,

$$BP + DQ = BR + DS,$$

that is,

$$BE + EP + DE - EQ = BF - FR + DF + FS,$$

or

$$BE + DE = BF + DF.$$

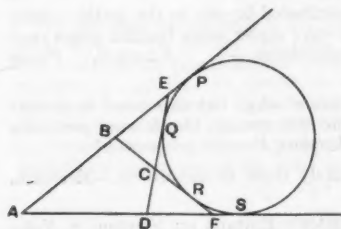


FIG. 2.

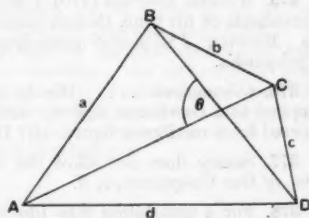


FIG. 3.

Thus, if a, b, c, d be the lengths of the consecutive sides of a deformable quadrilateral, we see that, whether it have a re-entrant angle or no, or even

if it be crossed, the condition that the four lines, of which its sides form parts, may be tangent to a circle, is confined to the three cases

$$a-b+c-d=0, \quad a-b-c+d=0, \quad a+b-c-d=0.$$

Since
$$(a+b+c+d)(a-b+c-d)(a-b-c+d)(a+b-c-d) \\ = (a^2-b^2+c^2-d^2) - 4(ac-bd)^2,$$

it is clear that the three cases may be included in a single condition.

Now consider the case of the general quadrilateral (Fig. 3).

We have
$$a^2-b^2+c^2-d^2=2AC \cdot BD \cos \theta.$$

Also
$$DBC-DAC=BDA-BCA=\phi, \text{ say,}$$

and
$$CAB-CDB=ACD-ABD=\psi, \text{ say.}$$

Thus the well-known vectorial relation,

$$(AB)(DC) + (AC)(BD) + (AD)(CB) = 0,$$

furnishes us with the relations

$$\frac{ac}{\sin \phi} = \frac{bd}{\sin \psi} = \frac{AC \cdot BD}{\sin(\phi + \psi)},$$

and therefore we have

$$ac - bd = AC \cdot BD \frac{\sin \frac{\phi - \psi}{2}}{\sin \frac{\phi + \psi}{2}}.$$

Thus the condition

$$(a^2-b^2+c^2-d^2)^2 - 4(ac-bd)^2 = 0$$

becomes

$$\cos^2 \theta \sin^2 \frac{\phi + \psi}{2} - \sin^2 \frac{\phi - \psi}{2} = 0.$$

So that we have the two cases

$$\cos \theta \sin \frac{\phi + \psi}{2} - \sin \frac{\phi - \psi}{2} = 0,$$

which reduces to

$$\tan^2 \frac{\theta}{2} = \tan \frac{\psi}{2} / \tan \frac{\phi}{2},$$

and

$$\cos \theta \sin \frac{\phi + \psi}{2} + \sin \frac{\phi - \psi}{2} = 0,$$

which reduces to

$$\tan^2 \frac{\theta}{2} = \tan \frac{\phi}{2} / \tan \frac{\psi}{2}.$$

If either of these conditions is satisfied for one conformation of the quadrilateral, it is satisfied for any possible conformation. J. BRILL.

839. [V. 1. a. ξ.] Having recently finished marking the Geometry of candidates in a certain well-known examination, perhaps I may be allowed a little space to record my impressions.

Script upon script there were, well over a hundredweight, and it took more than a fortnight of hard work to get through them all, and no one knows how much blue pencil! (By the way, does a blue pencil exist warranted not to break at one's angriest moments?)

As to the character of the work shown up, it could very easily be divided into the three classes of good, moderate, bad. Beginning with the last, the unfortunate ones who are incapable of appreciating or even comprehending geometrical reasoning, the "ἀγεωμέτρητοι" of Plato (over the entrance door of whose school were inscribed the significant words μηδὲς ἀγεωμέτρητος εἰσίτω), the type that makes a dash at a question and fails, goes on to another and fails, and so on through the whole paper, they were perhaps some eight per

cent. of the whole number. The moderates were, as usual, in the majority, though there were many others whose scripts entitled them to be called good. At least four or five floored the paper, showing up beautifully neat and intelligent work.

Why do not all teachers insist upon neatness and good handwriting? It would not only help to prevent mistakes, but would greatly facilitate the work of looking over and marking Geometry. Another thing that should be impressed on their pupils is the necessity, when turning over a page to complete a proposition, of making a fresh figure. It is more than tiresome for the examiner either to have to make another figure himself or to have to turn the page backwards and forwards continually. Exasperating also is illegible lettering of figures, D written like O ; M , N , and H indistinguishable. Still more exasperating is it to have to read carefully through proofs invented on the spur of the moment, getting further and further away from the point, and the argument at length lost in a maze of angles and lines.

"Propitiate your examiners" is what teachers should impress on their pupils, particularly in Geometry, by avoidance of these and similar shortcomings.

It was pleasant to see that some candidates had learned their Geometry from Euclid; wherever that was the case the work seemed to show better knowledge and fewer blemishes. The more, as a general rule, text-books adhere to Euclid's methods, cold and rigid though they be, the more successful are they; such, at least, is my impression.

No "howlers" cropped up in this particular lot of scripts, though one candidate produced a line till it "penetrated" the circumference of a circle.

A friend of mine similarly employed tells me that one of his candidates wrote on his (or, possibly, her) script the following appeal: "Dear Mr. Examiner, my best wishes for a happy Christmas and prosperous New Year. Please pass me."

Here was an attempt at propitiation indeed.

To conclude, taking it all round, my reflections lead me to believe that in most Secondary Schools, Geometry is carefully taught, and that the results of such teaching are better, to judge by this recent examination, than they would have been if a similar number of Public School boys had had to tackle the paper. And I am an old Public School Master. D.

840. [K¹. 6.] *Imaginary Circles in Cartesian Geometry.*

Definitions. "Point" means an ordered couple (pair of numbers), usually denoted by (x, y) . Note that there is no reference whatever to "axes."

A "real point" is a pair of real numbers; an "imaginary point" is a pair of numbers of which one or both may be complex.

"Circle" means a class of "points" satisfying an equation of the form $x^2 + y^2 + 2gx + 2fy + c = 0$; its "centre" means "the point $(-g, -f)$," and the "radius" is a number r defined by the equation $r^2 = g^2 + f^2 - c$. (In the special case in which r^2 is a real positive number, "radius" is further restricted to mean "the positive square root of $g^2 + f^2 - c$." The whole of geometry can be built up in this way; e.g. a "tangent" is a class of "points" satisfying a linear equation associated with an equation of higher degree in such a way that the two equations have a pair of equal roots, etc.

In this note an "imaginary circle" is the class of points satisfying the equation

$$x^2 + y^2 + 2(a + i\beta)x + 2(\gamma + i\delta)y + \epsilon + i\zeta = 0,$$

where (i) $a, \beta, \gamma, \delta, \epsilon, \zeta$ are all real, (ii) β, δ, ζ are not all zero.*

* I refrain from discussing circles with real equations which become imaginary for certain values of the coefficients (e.g. $x^2 + y^2 + 1 = 0$). In any case these belong to the comparatively uninteresting type which has real centres, but no real points or tangents. The circles I am discussing may, if a name be desirable, be called "essentially imaginary" circles.

TYPE I. *Centre real.* In this case $\beta = \delta = 0$, and $\therefore \zeta \neq 0$. The radius is a two-valued complex function of $a, \gamma, \epsilon, \zeta$.

Every point on the circle is imaginary. There are no point circles of this type, since one of the conditions for $r=0$ is $\zeta=0$, which is excluded.

TYPE II. *Centre imaginary.* The general case is that in which none of β, δ, ζ , is zero. If there exist real values of x, y satisfying the equation

$$x^2 + y^2 + 2(a + i\beta)x + 2(\gamma + i\delta)y + \epsilon + i\zeta = 0,$$

they must be solutions of

$$\begin{cases} x^2 + y^2 + 2ax + 2\gamma y + \epsilon = 0, \\ 2\beta x + 2\delta y + \zeta = 0. \end{cases}$$

These equations have two real solutions (which may be equal) or none.

Imaginary circles with real points. We have just seen that the centre must be imaginary. We may without loss of generality suppose one of the real points to be $(0, 0)$ and write the equation in the form

$$x^2 + y^2 + 2(a + i\beta)x + 2(\gamma + i\delta)y = 0.$$

The other real point is found from the equations

$$\begin{cases} x^2 + y^2 + 2ax + 2\gamma y = 0, \\ \beta x + \delta y = 0, \end{cases}$$

to be

$$x = \frac{2\delta(\beta\gamma - a\delta)}{\beta^2 + \delta^2}, \quad y = \frac{2\beta(a\delta - \beta\gamma)}{\beta^2 + \delta^2}.$$

This second point will also be $(0, 0)$ if

$$a\delta - \beta\gamma = 0.$$

In this case the circle takes the form

$$x^2 + y^2 + (\lambda + i\mu)(px + qy) = 0,$$

and, as we should expect, there is a real tangent at the origin.

Hence imaginary circles may have :

- (i) No real point.
- (ii) Two distinct real points.
- (iii) A real point and a real tangent.

Imaginary circles with real radius. The radius of the circle

$$x^2 + y^2 + 2(a + i\beta)x + 2(\gamma + i\delta)y = 0$$

is given by

$$\begin{aligned} r^2 &= (a + i\beta)^2 + (\gamma + i\delta)^2 \\ &= a^2 + \gamma^2 - \beta^2 - \delta^2 + 2i(a\beta + \gamma\delta). \end{aligned}$$

Suppose now $a\beta + \gamma\delta = 0$. We may write $\gamma = \lambda a$, $\beta = -\lambda\delta$, where λ is real, and not, in general, zero.

Then $r^2 = a^2 + \gamma^2 - \beta^2 - \delta^2 = (1 + \lambda^2)(a^2 - \delta^2)$, so that r is real provided $a^2 \geq \delta^2$.

We thus see that there exist imaginary circles having a real radius, and passing through two real points.

We cannot have $a\delta - \beta\gamma = 0$ and $a\beta + \gamma\delta = 0$ unless

- (i) λ is imaginary, which is excluded,
- or (ii) $\beta = \delta = 0$, which is excluded,
- or (iii) $a = \gamma = 0$, in which case r is imaginary.

Hence imaginary circles with a real tangent have their radius imaginary.

If, finally, we put $a\beta + \gamma\delta = 0$ and also $a = \delta$, we obtain $r = 0$. Hence there exist imaginary point-circles passing through two real points.

We cannot, in addition, have $a\delta - \beta\gamma = 0$ unless $a = \beta = \gamma = \delta = 0$, which is excluded. Hence there is no imaginary point-circle with a real tangent. The conditions that the radius of a general imaginary circle shall be real do not lead to any very striking result.

B. C. ATKIN.

841. [D. s. b.] *Exponential and Logarithmic Functions.*

$\pm \log_a x$ can be expressed to a required approximation by the ratio of two finite positive integers p, q .

Let $\log_a x = \pm p/q$,
so that $x = a^{\pm p/q}$.

If $b \neq 1$, we have the identity

$$\frac{b^p - 1}{b^q - 1} = \frac{b^{p-1} + b^{p-2} + \dots + b + 1 \text{ (to } p \text{ terms)}}{b^{q-1} + b^{q-2} + \dots + b + 1 \text{ (to } q \text{ terms)}}.$$

If now we put $b^q = \left(1 + \frac{1}{m}\right)^{\pm 1}$ and let $m \rightarrow \infty$, the right-hand side becomes p/q or $\pm \log_a x$. The denominator of the left side becomes $\frac{1}{m}$ or $-\frac{1}{m+1}$, and the fraction takes the form $\frac{0}{0}$.

Before making m infinite, we may write

$$b^p = \left(1 + \frac{1}{m}\right)^{\pm p/q} = \sqrt[q]{\left(1 + \frac{1}{m}\right)^{m \log_a x}}.$$

Hence we have $\text{Lt}_{m \rightarrow \infty} m(\sqrt[q]{e^{\log_a x}} - 1) = \log_a x$,

where e denotes $\text{Lt}_{m \rightarrow \infty} \left(1 + \frac{1}{m}\right)^m$.

If $a = e$, we then have $\text{Lt}_{m \rightarrow \infty} m(\sqrt[q]{x} - 1) = \log_e x$(1)

Hence $\frac{d}{dx} \log_e x = \text{Lt}_{m \rightarrow \infty} x^{\frac{1}{m}-1} = \frac{1}{x}$(2)

If we put $x = 1 + \xi$ in (1) and expand by the Binomial theorem, we easily find the expansion for $\log_e(1 + \xi)$. Alternatively we may integrate

$$\frac{1}{1 + \xi} = 1 - \xi + \xi^2 \dots$$

Putting $x = e^\xi$ in (2), we get $\frac{de^\xi}{d\xi} = e^\xi$, from which may be found the expansion for e^ξ and e .

WILLIAM MILLER.

842. [U.] *Caveant Emptores*

(1) Catalogue of important works on Astronomy, etc. Offered for sale by X. & Co., Ltd. Item 1316. WHARTON (Edith): *The Glimpses of the Moon*. 1922.

Admirers of *Ethan Frome* will not be misled. The book for sale is a novel, and has less to do with the moon than *A Midsummer Night's Dream*. I have come across the blunder before, but not by such distinguished perpetrators. Dare one suppose that X. & Co., far from napping, deliberately added to the number of lunar hoaxes? Cyrano de Bergerac, Jules Verne, and H. G. Wells would be at home in a scientific list, but if titles alone give a right of entry we shall look on the shelves of the R.A.S. for Kaye-Smith's *The Challenge to Sirius*—presumed to be an account of the companion—for Du Maurier's *The Martian*, for any book which describes *Le Roi Soleil*, and perhaps for Louys' *Aphrodité*!

(2) Catalogue of interesting books of the xvi, xvii and xviii Centuries Recently... added to... Stock... Z. & Co. Item 23. ASTRONOMY. *Pensées Diverses, écrites... à l'occasion de la Comète... de... 1680. 1699.*

That the handler of scientific books should not recognise the name of a contemporary novelist or know that the commas which make the pages seem

to him to be masses of quotations from unnamed authorities are actually being used under a different convention and indicate conversation, is unfortunate, but one's confidence in X. & Co.'s knowledge as specialists is not shaken. With Z. & Co. the case is different. The book is a famous work of its time, and really they ought to know all about it. The author was Bayle, compiler of the *Dictionnaire*, supreme master of insinuation, whose mind held the accumulated literary and scientific knowledge of his time, and whose pen decreed the future not of his language alone but of ours also. The subject is the belief in comets as omens, and while this led Bayle to many enquiries whose relevance is surprising, as for example whether atheism is worse than idolatry, the heading of astronomy is altogether inappropriate. The work was first published in 1682, and in 1704 appeared a long-promised *Continuation*.

E. H. N.

843. [U.] *A Bibliographical Mare's Nest.*

The list of authorities on the Calculus of Functions which occupies a footnote on the first page of the treatise on this subject composed by De Morgan for the *Encyclopaedia Metropolitana* (1836) opens with Babbage, "in various papers cited in this Work, and his *Elementary Treatise on the Calculus of Functions*." Some pages later there is a particular reference to p. 5 of the same treatise, and Sylvester, writing in French (*Comptes Rendus*, 94, p. 59; *Math. Papers*, iii. p. 564), describes a problem as "soulevé . . . par . . . Babbage, dans son traité *Sur le Calcul des Fonctions*." Nevertheless there is no treatise with the alleged title in the list of his writings which Babbage appends to his autobiographical *Passages from the Life of a Philosopher* (1864), and it is certain both that De Morgan is speaking of the work entitled *Examples of the Solutions of Functional Equations*, and that Babbage published no book to which the alleged title would be appropriate.

The cumulative evidence is conclusive. In the *Encyclopaedia Metropolitana* De Morgan does not refer in any other terms to the *Examples*, with which he was perfectly familiar, and in the *Penny Cyclopaedia* (xi, p. 15) he says that Babbage's *Examples* and his own article in the *Encyclopaedia Metropolitana* are "the only formal treatises on the subject, of which we know." The content of the explicit reference is peculiarly demonstrative; the reference describes a formula as "cited by Mr. Babbage from a paper by Mr. Horner in the *Annals of Philosophy*, November, 1817, which we have not seen"; on p. 5 of the *Examples* Babbage comments on the same formula that "a more detailed account of this method of solution may be found in a paper by Mr. Horner in the *Annals of Philosophy*, Nov. 1817," and while it is true that this is not "citing the formula," it is improbable that the same page of two different works would refer to Horner in identical words.

If Sylvester chose his words carefully, he also must have been referring to the *Examples*, for certainly Babbage wrote no treatise on the subject before the *Examples*, and the problem, which is in the *Examples*, could not well have been "soulevé" later.

It appears therefore that what we have to regret is not a masterpiece so rare that for practical purposes it is lost, but only a lapse from the precision with which we are accustomed to credit De Morgan in matters bibliographical.

The story of the origin of the *Examples* is told by Babbage himself (*Passages*, p. 39): "The progress of the notation of Leibnitz at Cambridge was slow. . . . It is always difficult to think and reason in a new language, and this difficulty discouraged all but men of energetic minds. I saw, however, that, by making it (the tutors') interest to do so, the change might be accomplished. I therefore proposed to make a large collection of examples of the differential and integral calculus. . . . I foresaw that if such a publication existed, all those tutors who did not approve of the change of the Newtonian notation would yet, in order to save their own time and trouble, go to this collection . . . to find problems to set. . . . After a short time the use of the new signs would

become familiar, and I anticipated their general adoption at Cambridge as a matter of course. I... communicated to Peacock and Herschel my view, and proposed that they should each contribute a portion. . . . Herschel prepared the questions in finite differences, and I supplied the examples to the calculus of functions. In a very few years the change was completely established; and thus at last the English cultivators of mathematical science, untrammelled by a limited and imperfect system of signs, entered on equal terms into competition with their continental rivals."

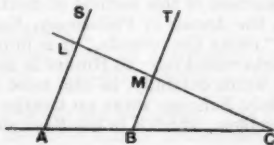
I have transcribed this artful tale for the sake of a second bibliographical puzzle which it explains. The three collections of examples were published in the same year, 1820, each with its own title, and there is no reference either on the title page or in the preface of any one of them to the scheme as a whole or to either of the other components. Herschel's collection and Babbage's were bound together, but paged separately, and the only indication in any individual copy that the binding of the two works in one volume was more than an owner's economy is the absence of any publisher's imprint from the title-page to Babbage's contribution. But the list of contents of Herschel's collection is headed "Part III," not because what was issued with Babbage's work was only the end of Herschel's, but because in Peacock's collection the examples on the differential calculus form "Part I" and those on the integral calculus form "Part II." Had Babbage's collection included anywhere the description "Part IV" the position would have been clearer, but his work has no subdivisions and no table of contents, and the heading to Herschel's table remains as a librarians' test of vigilance and knowledge, since it is easy to overlook it completely, and natural to catalogue the volume as imperfect on the strength of it.

E. H. N.

344. [V. 1. a. §.] *Direction and Parallels.*

One of the pitfalls for the writer of a text-book on Geometry is the idea that by the judicious use of the word "direction" he can avoid difficulties which would otherwise occur in the theory of parallel lines.

It is tempting to define parallel lines as lines that are *in the same direction*, and to explain that the line at *B* which is parallel to *AS* can be got by regarding *AB* as the standard direction and making *BT* differ by the same amount as *AS* from the standard direction by making $\angle CBT = \angle CAS$.



Unfortunately there is a hitch.

Suppose another line *LMC* cutting *AS*, *BT* at *L*, *M*.

Either we must know that $\angle SLC = \angle TMC$ or we have the absurdity that the lines *AS*, *BT* would not have been parallel had *LMC* been taken as the standard direction.

Thus it is entirely essential to the theory that "If two lines (*AS*, *BT*) make equal angles with one line (*AB*) cutting them, then they also make equal angles with any other line (*LM*) cutting them."

This is an alternative to "Playfair's axiom" or to Euclid's fifth postulate. Is it easier or more obvious than Playfair's axiom? It hardly seems so. At any rate it is an essential of the "direction" method.

If the writer who defines parallels by direction includes the statement (given

in italics above) prominently either as "postulate," "axiom," or even "experimental fact," his text-book may be worth consideration.

But if the writer omits it through ignorance he has scarcely studied Geometry sufficiently to write a text-book on it which is fit for use, and if he omits it deliberately he shows a levity and a lack of regard for truth which render him quite untrustworthy as a guide in any branch of Mathematics.

Moreover he has no excuse. The fallacy that the word "direction" makes the parallel-postulate unnecessary has often been exposed.

One of the most recent explanations of this is in the *M.A. Report on the Teaching of Geometry in Schools*, published in 1923.

If he prefers to go back further, C. L. Dodgson (*Euclid and His Modern Rivals*, 1879) deals with it very thoroughly.

If he feels the need of the authority of a great name, he can find out that Gauss wrote "If identity of direction is recognised by the equality of the angles formed with a third straight line we do not know without an antecedent proof whether this same equality will also be found in the angles formed with a fourth straight line." *

The necessity of including a version of the "parallels postulate" with the definition by "direction" does not condemn this definition. No definition of parallel lines escapes this necessity.

There appear to be three properties of parallel lines sufficiently fundamental to be adopted as defining them :

- (i) they do not cut ;
- (ii) they make equal angles with a transversal, i.e. are in the same direction ;
- (iii) they are everywhere equidistant from each other.

The number of possible substitutes, however, for Euclid's fifth postulate is very much greater, and their variety is very striking.

Who would recognise that the familiar forms of the postulate can be replaced by such assumptions as, "Given three points not in a straight line, there exists a circle passing through them" or "The area of any circle is greater than that of its inscribed regular tetragon" ?

But with the "direction" definition, the natural form of the postulate will deal with a second transversal, as in the version given above, and even from the purely didactic point of view the text-book writer must see that this second transversal is sure to be needed, and that some excuse must be made for taking the second pair of corresponding angles to be equal.

C. O. TUCKEY.

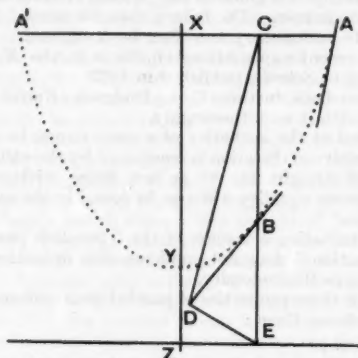
845. [V. a. μ .] *A Simple Form of the Catenary Experiment.*

In drawing up a course for the elementary mathematical laboratory an experiment which immediately suggests itself is that of the uniform chain suspended from two points, or catenary. A sheet of paper is placed on a drawing board held vertically behind the chain, and sufficient points are marked on the paper to determine the shape, the direction of the vertical being also obtained. The student has then before him the rather awkward process of identifying the curve and of determining the constant of the catenary c . To ease the calculation one of the supports may be replaced by a pulley with the free end of the chain hanging over it : the free end is then used to determine the directrix. This, however, may involve considerable error, for if a light chain be used the effect of the friction of the pulley becomes serious, while if the chain be thick and heavy the shape cannot be so distinctly obtained.

The following drawing method gives an easy solution of the problem with little calculation and high accuracy, and furthermore emphasises an important geometrical property of the catenary.

* Werke, IV. p. 365, quoted by Heath, *Euclid*, I. p. 194.

A fine chain is used (in the numerical case given each centimetre of it had 10 links and weighed .02 gm.). When points of the curve have been dotted on the paper at fairly frequent intervals (about 2 per cm.) and the direction of the vertical has been marked, the board is removed and the drawing process is completed as in the figure. Two points A, B are taken on the curve,



and the tangents through them are drawn (a good approximation is to take the join of two near points as giving the tangent direction for a point half-way between them). The horizontal through A meets the vertical through B at X , and the curve again at A' : the parallel to the A tangent through C meets the B tangent at D . Then, if the exterior bisector of the angle CDB meet CB at E , E lies on the directrix EZ , and the perpendicular distances of E from BD, CD equal c ; the perpendicular bisector of AA' is the axis, XYZ . The following table (in centimetres) indicates the subsequent measurements, the check lying in the approximate agreements of columns 2 and 4, 3 and 5; very slight reflection will show that it is the better practice to take the perpendicular distances of E from the tangents rather than ZY as the value of c .

$AA' = 2x$	c	$c \cosh x/c$	ZY	ZX
28.8	7	27.8	7.1	27.5

If a pulley be used, the weight of chain of length equal to the distance between the free end and the directrix clearly gives the frictional force exerted by the pulley in the particular setting.

H. G. GREEN.

846. [v. 10.] *Hindu Names.*

A Hindu name generally consists of two parts: (i) the actual name of the person, and (ii) the family surname. Thus take the name of our distinguished scientist, Sir Jagadichandra Bose. Here Bose is the family surname and Jagadir Chandra is the proper name. It is a compound word and should be written as one word. So in printing it in the European way—in the vernacular we use the full name—we should write Sir J. Bose, but it is generally written as Sir J. C. Bose. Sometimes other things are also included in the name, making it on the whole very complex. For instance, "Aiyar" (as in the case of Prof. Seshu Aiyar) is an adjunct of honour, somewhat akin to "squire."

BIBHUTIBHUSAN DATTA.

REVIEWS.

Abstracts of Theses. The University of Chicago Science Series, Vol. I., 1922-1923. Pp. 1-500, with Author-index. n.p. (Chicago University Press, 1925.)

This volume opens a proposed series wherein will be published abstracts of theses presented for the Doctorate of Philosophy in the Chicago University Graduate School of Science. The present book covers the academic year 1922-1923 together with certain earlier work. It includes the usual departments of mathematics and natural science, together with medical and educational theses.

Mathematics occupies sixty pages shared among eight abstracts, with a list of five other papers published elsewhere in full. Astronomy and Physics fill the next fifty pages or so, and Chemistry another hundred and fifty. This involves rather more than half the book. Geology, geography, zoology, anatomy, and so on, are represented in the remaining pages. It is evident that Chicago is an important centre for mathematics, physics and chemistry; and the present volume certainly helps to confirm the view that Chicago ranks with Harvard as a leading mathematical centre in America.

Because of the enormous development and output of new mathematics and science, abstracts and summaries are useful. Moreover, there are clearly two ways of systematically dealing with them: either by publishing accounts of new results according to subject (as in *Science Progress*, or in the specialised and more complete mathematical *Jahrbuch*), or by geographical origin as in this Chicago collection. The former method is indispensable, the latter is something of a luxury, only possible when plenty of money is forthcoming. It furnishes a useful guide to what is taught in the particular University, and this is perhaps of most value to students who desire to avail themselves of facilities for research in foreign countries, and therefore wish to know the dominating influence of this or that intellectual centre.

Thus Chicago is the algebraic centre of America; and the influence of Dickson in algebra and of Moore in the logical basis of pure mathematics is clearly reflected in these abstracts. Also we find the field of the real variable and the calculus of variations represented; evidently the influence of Bliss. Chicago, too, is an important centre for applied mathematics and astronomy, although the present reviewer cannot speak with the same confidence here, but depends on the opinion of others more qualified to judge.

There are two abstracts of papers on the logical basis of mathematics, part of the systematic work going on in America, following the lines of Whitehead and Russell. First Ballantine writes on the *postulational introduction to the four-colour problem*. The problem is a curious little puzzle in abstract geometry. Can a map of counties be properly distinct if four and only four colours are used? Tait answered it mathematically, though "his proof is not entirely satisfactory." The author claims to have found the exact assumptions necessary for an affirmative answer, and to show that the problem is a special case of a theorem conditional for the solution of a set of linear equations. Certain other particular results are stated.

Next Van Horn writes on a *system of relative existential propositions connected with the relation of class membership*. This seems to be an important paper, but how far it is original work is not explicitly stated in the abstract; it certainly looks original, following on and enriching results of Zermelo published in the *Math. Annalen* 59 (1904) and 65 (1908). The main thesis of the paper is so interesting that it is perhaps worth while explaining a little. We are indebted to Dedekind among others for the precise definition of an irrational number, like $\sqrt{2}$, as a class of all rational numbers r with a specific property, e.g. that $r^2 < 2$. It is this word class which is important, and its fundamental rôle in mathematics was fully brought out by Peano. We may say that a class P^0 has members typified by p^0 , and each number belongs to the class. Also any group of members p^1 short of the whole class is a *sub-class* of P^0 . We may now think of the class P^1 whose members are typified by p^1 , that is the class of all subclasses of the original class. This process may

go on and be elaborated through P^1, P^2 to P^m . The author makes this his subject-matter, and builds up a consistent series of axioms. He gets as far as a multiplicative axiom. In the choice of his theme for study we seem to see the influence of the theory of groups, and their subgroups. In other words, the influences of Dickson and of Moore mingle for the suggestion of themes for research students.

Other abstracts are by Clarke, *on the minimum of the sum of a definite integral and a function of a point*; and by Ehleman, *on the Lagrange problem in parametric form in the calculus of variations*; and by Wilson, *on representations of certain functions of two variables by Stieltje's integrals*. These represent the contribution to analysis; in each case the results are clearly stated. There are three dynamical papers, two on problems suggested by the Relativity hypothesis. One is quoted by title on Line Geometry; four others on modular invariants, while two on this last subject are given in abstract. The first is by Yanney *on modular invariants of the binary quartic*; the second is by Zeisler *on definite integral representation of invariants*. We are here in touch with the most characteristic work of the Chicago school, the development of modular invariants. This takes its rise in a memoir,* on the general theory of modular invariants, by Dickson in 1909. It is an enormous subject which, briefly stated, is the outcome of using the concepts of projective invariants in the realm of the theory of numbers. Cayley, Salmon and Sylvester developed the projective invariant theory in relation to ordinary algebraic equations. Dickson developed the analogous theory for modular equations. Thus

$$ax + b = 0, \dots\dots\dots(1)$$

$$cx + d \equiv 0 \pmod{N} \dots\dots\dots(2)$$

are examples of linear equation, and linear modular equation. In (1) the coefficients a, b may be real or complex numbers. In (2) N is usually a positive integer, and c, x, d are integers prime to N . There is here a clear case of parallel theories, becoming still clearer when the left-hand sides of (1) and (2) are made homogeneous in x, y as $ax + by, cx + dy$. The degree of the form (linear here) survives a linear transformation of variables x, y in both cases. Invariant functions of the coefficients exist in both cases when the form on the left is of degree greater than one in x, y .

In the thesis by Yanney, the case when N is the n th power of a prime such that

$$N \equiv 1 \pmod{8}$$

is considered, for the binary quartic. The abstract gives satisfactory statements of how the results are arrived at. Needless to say the problem is exceedingly more complicated than the ordinary algebraic theory of the quartic, although that provides the basis. The modular theory is now in a position analogous to that of the original theory in the middle of last century.

To judge from these abstracts one may get a fair perspective of what is going on in Chicago University, and this systematic publication yearly will be quite useful.

Out of curiosity one dips into the other sections of the book and finds with some satisfaction that the language of mathematics, contrary to prevalent opinion, is nearer to that of everyday use than is the language of other sciences. On p. 249 it is found convenient to call a carbon compound, denoted by $(C_6H_5C_6H_4)(C_6H_5)_2CNHCl$,

DIPHENYLMONOBIPHENYLMETHYLCHLOROAMINE,

a name reminiscent of a menu in Aristophanes or a village in Wales rather than the laconic utterances of America. At the end of the book is an interesting thesis on catching and avoiding colds. *The common cold: an etiologic study*. Over 900 college students supplied the data for a survey of this tiresome but very insistent problem. Alas, the result of a thorough-going

* *Trans. American Math. Soc.*

survey leaves us where we were before. On the basis of these statistics, some catch colds, a few do not. Open or closed bedroom windows make no difference. Germs lodge in the throat ("can acquire invasive capacity"). A chill may follow violent exercise. Other people with colds are dangerous. These are the conclusions.

Aufgaben und Lehrsätze aus der Analysis. By G. PÓLYA and G. SZEGÖ. Band I. pp. xvi + 338. Band II. pp. 407. 15 goldmark. (Bound, 16-50 goldmark.) 1925. (Springer, Berlin.) I. *Reihen, Integralrechnung und Functionentheorie*. II. *Functionentheorie, Nullstellen, Polynome, Determinanten und Zahlentheorie*.

These two volumes of examples in analysis are numbered XIX. and XX. respectively in the *Grundlehren der mathematischen Wissenschaften* series, which is producing so many interesting works. The volumes under notice together form an organic whole. They are the result of collaboration of writers whose work lies in Zurich and Berlin respectively, while Professor Pólya himself is a member of the London Mathematical Society. To judge from the contents of the books they represent an enormous teaching experience over a wide range of advanced subjects, generally comprised under the title Analysis: and one is not surprised to find in the preface a long list of the names of most of the eminent Continental mathematicians who have in one way or another materially helped in the compilation and verification of the subject-matter. To these many helpers the authors acknowledge their indebtedness; but the chief feature of the whole book, which only reveals itself with systematic reading, is the undoubted skill shown in the choice and grouping of the examples. The care and thought with which this is done, render the authors' own contribution something far different and more important than merely marshalling disjointed exercises into one great compendium.

The plan of the book is quite simple. The first half of each volume consists of sets of examples; the second half contains answers and, where necessary, outline solutions. Most of the examples are enunciated in the accepted form, but occasionally an opening example in a new section of the work is more of a carefully formulated explanation, or definition, than an actual exercise. After the preface there is a useful list of abbreviations and technical terms. Then the plunge is made into the examples—there are in all over sixteen hundred of them—and each volume closes with a useful index, the final one giving, in suitable subdivisions, names, technical terms and subject-matter.

It will be seen that the work is in no way a treatise on Analysis; nor is it meant to be. At the same time it is no mere bundle of exercises. For the unit here is not so much the single exercise as the family of exercises, based literally on one dominating idea which binds them into an organic whole. Such a family is marked off from the kindred families, which go to form a chapter in one section of the book. There are nine such sections. So the whole object of the book, as the authors explain in the preface, is to give the reader systematic opportunity to master the main principles of the various important branches of Analysis.

The contents of the book are as follows: Section I. deals with infinite series and sequences, in four chapters, devoted respectively to power series, transformations, the structure of real series, and miscellaneous consequences. Section II. contains the integral calculus, starting with the Riemann-Darboux definition of a definite integral, for Chapter I.; then a chapter on inequalities (showing the inherent unity of a great deal of work in fairly elementary algebra and in more advanced analysis, which students generally acquire rather at random). Then come chapters on real functions, and various problems on limits of sums and products, and so on, all in a general way connected with the starting-point of the section. Some of the connections are obvious, as that the theory of convergence of an endless series of terms implies a correlative theory for certain endless products of factors. One curious little example occurs: to prove the number

· 1 2 3 4 5 6 7 8 9 1 0 1 1 1 2 1 3 ...

is irrational. The digits are those of the positive integers written as they would be spoken if one were counting.

Section III. is devoted to the complex variable, bringing in with it new ideas, as well as illustrating the general body of fundamental theory found in most text-books on the theory of functions of the complex variable. This section completes Volume I., the guiding principle throughout being to elucidate the essential general principles of Analysis.

In Volume II. certain more specialised matters are considered. Section IV., continuing III., deals first with an aspect of power series, only recently developed systematically. Suppose such a series

$$f(z) = a_0 + a_1z + a_2z^2 + \dots + a_nz^n + \dots$$

to converge within a circle of radius R . Then, for a radius r less than R , the series of moduli whose n th term is

$$|a_n| r^n$$

converges. If $\nu(r)$ is the index belonging to the greatest of these terms, this is called the *central index*. When several such terms are equal the index of the most advanced of these, towards the right in the series, is chosen. From this, as a sort of *origin*, the series is now contemplated, and the idea is developed in a set of seventy-six examples, some quite easy, some distinctly difficult. After various other aspects of the function-theory are dealt with, the theory of equations claims Section V. Section VI. exemplifies polynomials and trigonometrical polynomials; VII., determinants and quadratic forms; while VIII. has five chapters on the theory of numbers, and the work closes with IX., a handful of geometrical examples.

Everything is done to make the work of practical value. Abbreviations are fully explained in the opening pages of each volume. Any special name or topic can be readily found from the index, though it is curious that *limit* is omitted from this. Also, throughout, there are ample cross references and hints as to the origins of isolated results or of more elaborate groups of results. These in themselves are valuable, both for the clear light so readily thrown on the actual contributions of early masters of Analysis, and also for the information here given of many quite recent advances and tendencies in analytical thought.

The book presupposes a certain elementary knowledge of Analysis. Probably its most useful field, at any rate in England, would be among all who are called upon to teach or lecture to Honours students. It is essentially stimulating and suggestive. It has a peculiar atmosphere of making the reader—who, of course, has to read with a pen in his hand—ask himself again and again, Have I really grasped the underlying thought below these exercises? Most of us are content to solve a problem by any means, and only the rare spirits make it their habit to criticise their style amid the welter of analytical technicalities. The claim that is modestly made in the preface (which itself is extremely interesting reading) that the book ushers in an entirely new mode of presenting examples for solution, would seem to be true.

The book can be warmly recommended.

Algebra for Schools. Part II. By J. MILNE and J. W. ROBERTSON. Pp. 175-299, xxxvii-lil. 2s. 6d. net. 1926. (London: G. Bell & Sons.)

Part I. was published in 1923, and has already been reviewed in the *Gazette*. The two parts, which together give a course suitable for Secondary Schools, cover the elementary ground up to and including the binomial theorem for a positive integral index. The book resembles quantities of other books like it which have been published since the reign of Hall and Knight towards the end of last century, and it is not easy to see why still another should be published.

For its size it is well proportioned, and a schoolboy keeping steadily to its guidance would learn something of algebraic technique. A good point is its explicit declarations that some things are too hard to prove at the stage reached, although they may be worth using. For instance, the short explanations of the theory of indices and that of the approximate formula, when x is small,

$$(1+x)^n = 1 + nx,$$

are good. A special sign is used here instead of $=$, namely \simeq . But the treatment of the infinite series in geometrical progression seems bald. The real difficulty is shelved, and the impression is left that the writers insert a few instances of the use of the method of limits without being really interested in so doing. What is the cogency of the argument that $(\frac{1}{2})^n$ becomes smaller and smaller as we increase n , because it does so when expressed as a decimal? This occurs on p. 242. Why not adopt the categorical method used for the theory of indices, stating explicitly the assumption of the theorem that if r is a proper fraction r^n tends to zero as n increases indefinitely?

One sometimes laments the passing of such books as Todhunter's *Algebra*. They had their defects, but what they did well was explained far better than in their present-day abbreviated successors, which are often little more than rather jerky series of notes.

H. W. TURNBULL.

Binomial Factorisations. By LT.-COL. ALLAN J. C. CUNNINGHAM, R.E. Vol. III. Pp. lxxix+203. 15s. Vol. V. Pp. lxxii+120. 15s. Vol. VII. Pp. 117. 15s. 1925. (London: F. Hodgson.)

Four volumes of this work have already been noticed in the *Gazette*; Vols. I. and IV. in July 1924, and Vols. II. and VI. in March, 1925.

The rapid completion of this set of books is little indication of the labour expended in preparing them. For more than thirty years the author has interested himself in systematically recording the prime factors of numbers of the form $a^n \pm 1$, and the concentrated result of his labours is contained in the *Binomial Factorisations*.

One illustration, defined by $y^4 + 1$, has been given in an earlier review: for another we take $y^7 \pm 1$ from Vols. III. and VII. A prime factor of $(y^7 \pm 1)/(y \pm 1)$ is necessarily $\equiv 1 \pmod{14}$. Col. Cunningham first takes each prime $14i + 1$, which $\nmid 10^4$, and tabulates, in order of numerical magnitude, the roots of the congruences

$$\frac{y^7 + 1}{y + 1} \equiv 0, \quad \frac{y^7 - 1}{y - 1} \equiv 0 \pmod{p}.$$

After this preparation he is able to pick out the values of p which correspond to a given y , and hence to give the prime factors, not exceeding 10^4 , of $(y^7 \pm 1)/(y \pm 1)$. For many values of y , < 251 , the factorisation is complete, e.g.

$$(223^7 - 1)/(223 - 1) = 29.491.1709.5076443,$$

$$(223^7 + 1)/(223 + 1) = 7.43.211.6553.294169.$$

An offshoot of these tables is the application to factorisation of the numbers termed Aurifeuillians which have been specially studied by Col. Cunningham. Taking $y = 7\eta^2$, $(1 + y^7)/(1 + y) = (1 + y)^6 - 49\eta^2(1 + y + y^2)^2$, and an algebraic decomposition is possible. These numbers have been examined up to $\eta = 49$, the factorisation being complete in many cases, e.g. $\eta = 39$,

$$(1 + y)^6 + 7\eta(1 + y + y^2) = 29.14197.3007481,$$

$$(1 + y)^6 - 7\eta(1 + y + y^2) = 39019.30147349,$$

the larger factors being prime, since they have no prime factor $14i + 1$ less than 10^4 .

A few other special tables are added, mainly of interest as curiosities in factorisation.

In the whole set of volumes Col. Cunningham shows how much information he has been able to obtain in his systematic way about the integral factors of $a^n \pm 1$. His tables also show how limited yet is our knowledge of the factors of a large number. Thus, after all Col. Cunningham's labour, no information can be given about the prime factors, if any, of $(y^7 + 1)/(y + 1)$ when $y = 218, 220, 221, 224, 226, 227$, and 228. Col. Cunningham's work is truly a monumental one, and will long remain the standard one on the subject, though isolated results, where it contains gaps, will doubtless be discovered from time to time.

Factorisation of $y^n \pm 1$ ($y = 2, \dots, 12$) up to high powers of n . By Lt.-Col. CUNNINGHAM, R.E., and H. J. WOODALL, A.R.C.Sc. Pp. xx+24. 10s. 1925. (London: F. Hodgson.)

This, in a way, is the most satisfying of Col. Cunningham's recent books. In the *Binomial Factorisations*, already noticed in these pages, he gives the

prime factors, as far as they have been determined, of $a^n \pm 1$ for wide ranges of values of a and certain small values of n , such as 7, 12, etc. Here he and Mr. Woodall record the primes dividing $a^n \pm 1$ when $a=2, 3, 5, 6, 7, 10, 11, 12$ for a range of values of n . Tables for $a=2, 10$ previously given by other investigators have been brought up to date by many new additions. The rest is mainly due to the authors' own work.

From the tables under $a=2$, which are extended as far as $n=500$, it appears that $2^{117}-1, 2^{135}+1$ are the largest numbers $2^n-1, 2^n+1$ that have been completely factorised. The tables extend to $n=100$ for the other values of a .

In previous tables Col. Cunningham has given the least solution x of the congruence

$$a^x \mp 1 \equiv 0 \pmod{p}$$

for each prime p less than 10^5 . Reference to these results has enabled the authors to pick out the primes, less than 10^5 , dividing $a^n \pm 1$. Such a method is a perfectly systematic one, leaving nothing undone within the limits of the tables. Thus the authors are able to say that all factors $< 10^5$ have been cast out: for example,

$$7^N + 1 = 8.179.3917.18691.N,$$

where N contains no prime factor having less than six digits. When $a=2$, the prime factors less than $3 \cdot 10^5$ are recorded.

The volume also gives all known information about Mersenne's and Fermat's numbers. Père Mersenne, in 1644, affirmed that, when q is a prime not > 257 , $2^q - 1$ is composite except when

$$q = 1, 2, 3, 5, 7, 13, 17, 19, 31, 67, 127, 257.$$

The grounds of this statement are not known: possibly it was a conjecture, but it has been shown to be wrong in only four cases, when

$$q = 61, 89, 107 \text{ and } 67,$$

the first three of these values making $2^q - 1$ prime, whereas

$$2^{67} - 1 = 193707721.761838257287.$$

Ten values of q still remain unsettled.

Fermat stated that he believed $2^f + 1$ to be prime when $f=2^n$, another conjecture which has been shown to be wrong for more than a dozen values of n . Readers interested in this line of thought will greatly appreciate the present concise tables giving all the results known up to 1925.

A method of factorising $a^n \pm 1$, distinct from Col. Cunningham's systematic one, is sometimes available, though possibly its application is limited. If

$$a^n - 1 = a\beta, \text{ where } a - \beta = a^s,$$

then

$$a^{2n-2s} + 1 = (a^{n-s} + a)(a^{n-s} - \beta).$$

From this and similar identities two factors of $a^n \pm 1$ can be deduced from two factors of $a^n \pm 1$ in certain cases. Thus from

$$2^8 - 1 = 9 \cdot 7, \quad 9 - 7 = 2,$$

we have

$$2^{10} + 1 = (2^5 + 9)(2^5 - 7) = 41 \cdot 25;$$

and since

$$41 - 25 = 2^4,$$

it follows that

$$2^{11} - 1 = (2^5 + 25)(2^5 - 41) = 89 \cdot 23.$$

Again, from

$$10^3 - 1 = 37 \cdot 27, \quad 37 - 27 = 10,$$

we deduce

$$10^4 + 1 = (10^2 + 37)(10^2 - 27) = 137 \cdot 73. \quad \text{W. E. H. B.}$$

A Course of Pure Mathematics. By G. H. HARDY. Fourth edition. Pp. xii, 449. 12s. 6d. 1925. (C.U.P.)

Prof. Hardy's book needs no words of mine to recommend it, and I shall not be thought blind to its many well-known merits if I take the occasion of welcoming a new edition, at a price, be it noted, only sixpence above the price in 1908, to discuss a detail. The one important change that the author has made, which is to transfer to the text an account of an analytical basis for the theory of the circular functions, prompts the consideration of the relation of elementary geometry to elementary analysis. Every learner for whom such a course as this is designed is acquainted, in some sense or other, with theorems about triangles and circles, and the writer on analysis may

accept this knowledge uncritically, ignore it, allow geometrical results to suggest possibilities for analytical investigation, or indicate how the subject can be established on a strict basis in some form relevant to his needs.

The first course leads to the royal road of pictures and plausibility, which it is the very object of Prof. Hardy's teaching to discredit. When we consider what the second course involves, we may doubt both whether it could be adopted successfully at the undergraduate stage and whether it is really the analyst's ideal. The first point need not be laboured: to use familiar trigonometrical language and yet to know that none of the implications of that language is being utilised demands a certain maturity. As to the second point, the domain of the complex variable, however approached, is a two-dimensional continuum, and the mathematician is not going to say that when he calls himself a geometer in studying this continuum he necessarily accepts a standard of reasoning which as an analyst he regards as unworthy of confidence.

If geometry is not repudiated, retrospective analytical proofs still form a delightful exercise, and it is particularly important that a generation whose teachers are so alive to the difficulties of inverting a complex integral that the Jacobian elliptic functions have practically disappeared from the elementary programme, should at least see how for a real variable the circular functions would be demanded for integration even if they were not otherwise known.

The conclusions I wish to emphasise are that sooner or later the student must learn that geometry is a rigorous branch of mathematics, and that in developing analysis seriously, just as we say enough of the theory of real numbers to make clear the nature of the problem and of its solution without giving an elaborate formal proof of the distributive law, so at the appropriate time we can explain that deductions from elementary geometry are valid without interrupting our work to make a premature study of foundations.

All that I have said is in agreement with Prof. Hardy's procedure. It is to similar triangles that he turns for a suggestion for a rule for multiplying complex numbers, and he takes familiarity with sines and cosines for granted long before he hints at any difficulties lurking in their use. But when he does refer to the problem of dependence on geometry, he criticises "the unproved assumption that angles are capable of numerical measurement", and answers the question, "What is the x in $\sin x$?" by defining the measure of an angle in terms of the area of a sector; it is here that I want to quarrel.

If we reduce to a tautology the theorem that the area of a sector is proportional to the angle, we certainly avoid a troublesome demonstration. But what have we made thereby of the assertions that an angle AOB at the centre of a circle is double an angle APB at the circumference and that the external angle of a regular pentagon is four-fifths of a right angle? As commonly interpreted and proved, the first of these assertions means that there is a radius OC in the angle AOB which is such that each of the angles AOC , COB is congruent with APB , and the second means that if AOB is one of the external angles and AOC is a right angle, then AOB can be divided into four parts congruent with each other and with BOC . Elementary geometry can not dispense with results of this kind, but if measurement of angles is to mean measurement of areas, these results can not be enunciated in the classical manner; the classical enunciations acquire complicated meanings, and to prove that with these meanings the enunciations are valid is virtually to reproduce the demonstration which the tautology has tempted us to think superfluous.

If elementary theorems on congruence are accepted—and Prof. Hardy seems willing to accept them—the statement that one angle is m/n times another angle, where m and n are whole numbers, has a primitive meaning, illustrated in the two theorems I have quoted, as direct and inevitable as the statement that one rectilinear length is m/n times another rectilinear length, and since the statement that one angle is less than another is intelligible also, it follows that given any two angles α , λ of which the first is not zero, the class of rational numbers to which x belongs if and only if $x\alpha$ is less than λ is a definite class. In other words, the general theory of measurement by real numbers is immediately applicable to angles as well as to rectilinear lengths, not merely

without unproved assumptions as to the existence of particular curvilinear lengths, but in a form in which no distinction has to be drawn between euclidean and non-euclidean geometries. If direct measurement is possible, derivative measurement may still be preferred if substantial advantages are secured, but in the present case I find myself wondering how heavy would be the task of showing in detail that on his own sequence of definitions Prof. Hardy has the right to quote the formula for $\sin(x+h) - \sin x$ when he wishes to differentiate $\sin x$.

If angles are measured directly, the unit is arbitrary. The ratio $(\sin x)/x$ has a limit k dependent on the unit, and the derivatives of $\sin x$ and $\cos x$ are proved by the usual argument to be $k \cos x$ and $-k \sin x$; the radian is the unit for which k is unity, and it is because the advantages of having unity for the value of k are overwhelming that the mathematician, though he rarely wants actually to measure arcs or sectors, uses circular measure always, not only in studying analytically the functional relations which are those of the trigonometrical ratios to a measure of the angle, but also in investigating non-euclidean geometry and the geodesic geometry of a surface in euclidean space.

Far from being of importance in Prof. Hardy's plan, the paragraphs bearing on this question could be deleted altogether without leaving an obvious gap. I have made a mountain out of an accidental molehill in a way that would be unpardonable except on the assumption that readers of the *Gazette* do not need to be told what kind of book this is. But to allow for the possibility, wildly improbable though it is, of a reader unacquainted with the book, let me add that in scope the course is an introduction to analysis, which explains with fascinating clearness and with an abundance of illuminating examples the meaning of limits in general and of sums and derivatives and integrals in particular, and culminates in a theory of the logarithmic and exponential functions of the complex variable, and that in style the course is an unsurpassed education in exact thinking, which has played and will long continue to play a notable part in setting a high logical standard for the early training of mathematicians in this country.

E. H. N.

Le Problème de Pappus et ses Cent Premières Solutions. Par A. MAROGER. Pp. viii + 384. 25 fr. 1925. (Vuibert, Paris.) Avec une préface par Prof. MONTEL.

Pappus' problem is: Through a given point on the bisector of a given right angle, to draw a line on which the arms intercept a given length. This leads to a quartic equation, soluble by square roots, and therefore to a ruler and compass construction.

Variation of the axes, coordinates or parameters, order of elimination and choice of elements for geometric interpretation give rise to not a hundred, but an infinite number of so-called different solutions; for example, by constructing any two curves through a set of four essential points of the figure and discarding their irrelevant intersections. Naturally, each solution gives the same answer, the same condition of reality, and much the same analysis. The interest in the main subject is exhausted by half-a-dozen of the most dissimilar lines of approach; but embedded in the mass of slighter variations, one is pleased to find some unlikely properties of the strophoid and lemniscate, and one must admire the author's ingenuity, perseverance and delight in his subject.

Very little previous knowledge is assumed, and the necessary explanations of standard methods, such as inversion, both theoretical and mechanical, are clear and brief. A single section might well suggest a stray lecture for a class needing a change.

The preface contains a comparison with Klein's *Ikosahedron*, which is less kind than was intended; Klein knew when to stop.

H. P. H.

A Treatise on Hydromechanics. Part I. Hydrostatics. By W. H. BESANT and A. S. RAMSEY. 9th Edition (revised). Pp. viii + 134. 7s. 6d. net. 1925. (Bell.)

The new (9th) edition of this well-known book is considerably reduced in

size, from 234 to 132 pages, the object of the revision being to come into line with changes made in recent years in mathematic studies.

Three short chapters have been entirely omitted, namely, those on Oscillations of Floating Bodies, Tension of Flexible Surfaces, and Laminas subject to Fluid Pressure. Among other things our old friend the lintearia disappears. The number of examples is cut down by more than one-third, mainly by the omission of a number of artificial ones, e.g. the type where things happen to paraboloids in liquids of curiously varying densities. The chapter on Capillarity has been considerably shortened and does not lose by the changes. These alterations account for the bulk of the reduction in the number of pages in the book.

The student who learns his Hydrostatics from this edition will be none the worse for not having to do some of the elaborate problems which appeared in the previous editions and in other works on this subject.

It is an encouraging sign of the times that the reviser does not feel that a proof of Green's Theorem is now necessary, as he uses it by mere quotation in the second chapter.

W. M. R.

A School Mechanics. By C. V. DURELL. Part II. Pp. xvii + 187-322. Part III. Pp. xxvi + 323-447. 3s. each. 1925. (Bell & Sons.)

Part I. was reviewed in the January number of the *Gazette* (p. 290).

Part II. starts with Newton's Laws, and treats of the composition and resolution of velocities, accelerations, and forces, the equilibrium of a rigid body, centre of gravity and stability, and motion in a circle.

Part III. consists of applications of dynamical principles to waterwheels and turbines, collisions, projectiles, bending moments and shearing, graphical statics, harmonic motion and pendulums, and two-dimensional motion of rigid bodies.

Parts I. and II. cover the ground of the School Certificate and similar examinations. Part III. contains the additional work required for the Higher School Certificate, Mechanical Science Tripos, and Entrance to Woolwich. In these volumes the plan is continued of giving revision notes at the beginning of each volume of the work contained in the volume, and the tables are repeated on the cover of each volume.

The author is not quite happy in his discussion of absolute and gravitational units at the beginning of Vol. II., and discards absolute units at the earliest opportunity, confining himself thereafter to the units required in Mechanical Engineering. He never allows his symbols to represent quantities: they are always numerical coefficients of specified units. This seems a pity, but it is at any rate consistent.

The examples are excellent and well graduated and very numerous: a storehouse of practical and academic problems. As a rule the explanations are clear and accurate, but there are a few that seem to need emendation. Thus in II. xv. (iii): "If the speed is variable, the radial acceleration . . . varies." Surely this should be "exists, and *may* vary." The proof on p. 252 that if a rigid body is in equilibrium under three forces, AB , CD , EF , they must be coplanar is far from convincing. There is no proof that AB , CD are coplanar, and if EF is parallel to his imagined hinge line HK it need not cut either AB or CD so far as the proof shows. As the work deals almost entirely with coplanar forces, this little excursion into three dimensions might be omitted as beyond the scope of the book.

The second paragraph on p. 271 is not of much use. It does not prove that the centre of gravity might be at G with one position of the body, and with other positions at G' , but only that it cannot be *simultaneously* at both places, a peculiarity which no one could imagine possible! The first paragraph is sufficient. Of course the whole theorem depends on the body being small enough for the attraction forces to be all taken as parallel.

The only misprints noticed are ft./sec. for acceleration on p. 297; and probably a wrong sign in the numerator in the value of x on p. 370, Ex. 22, which should, I think, be $a(l + \mu^2 h)$, not $a(l - \mu^2 h)$. It would be a help to the student, in this example, if the mode of fixing the guy-rope were explained, as also the meaning of the lower arrow P .

The Tutorial Statics. By W. BRIGGS and W. H. BRYAN. 4th Edit. Pp. viii + 366. 5s. (W. B. Clive.)

A very well arranged book, with clear explanations and good examples: well suited to a beginner who has a working knowledge of Trigonometry. It deals with the foundations of the subject, simple machines with and without friction, centres of gravity, elementary graphic statics, and virtual work. The diagrams are clear, particularly in connection with pulleys and screws. On p. 176 it would have been better to show the chain which goes round the differential pulley system as being endless: the hanging end seems very strange and helpless. But this is a small blemish in a remarkably good foundational book.

A. LODGE.

Elementary Integral Calculus. By G. LEWINGDON PARSONS. Pp. ix + 127. 5s. 1926. (Cambridge University Press.)

This book opens with a good historical sketch. The first five chapters are concerned purely with the technique of finding indefinite integrals, the next four chapters with the summation aspect of integration, and the last with the solution of a few types of differential equations. The examples worked out in the text are well chosen, there do not seem to be any misprints, and the book is attractively presented by the publishers. It is a good crammer's book and, as such, may prove useful to older students. But it is quite unsuitable for school use, as indeed any book on Elementary Integral Calculus must be. Introductions to the Calculus may be written, and many have been written, for schools. In such books it is vitally necessary to explain and illustrate the principles of both aspects of the Calculus using the minimum of Algebra, and then to employ a more advanced technique. In the Preface to this book the author writes: "No attempt has been made to treat the subject from an absolutely mathematical standpoint." And it is clear that he has not written the book from an educational standpoint.

Plane Trigonometry. By P. R. RIDER and A. DAVIS. Pp. xi + 272. 6s. net. 1926. (Blackie.)

In the preface the authors state that one of the features of this book is its completeness. The first nine chapters are, indeed, a very complete account of Elementary Trigonometry, but the last chapter on Analytical Trigonometry is very scrappy; as indeed it must be, since it consists of only 38 pages. It might well have been omitted. A very pleasing feature of the book is the four portraits and short biographies of Euler, Napier, Vieta and Descartes. There are also many interesting historical notes, while the practical illustrations quoted are numerous and varied. There is some looseness of statement here and there—e.g. on p. 12, π is "a trifle less than 3.1416," and on p. 20, the statement that the mil is "a very close approximation to the thousandth part of a radian"; as a matter of fact, the latter is 1.86 per cent. greater than the former. But the authors state that the mil found great favour in the War, and we all know the story of the sergeant who said that the circumference of a circle was exactly three times its diameter—except, perhaps, for very big ones. Some of the language used in this book is unfamiliar to English ears, e.g. "csc" for cosec on p. 45, "stricken out" on p. 64, and "scratch paper" on p. 99. There is one very bad misprint in formula (149) on p. 271. The print is attractive and the diagrams are clear, while there is a very complete index. American text-books are said to be noted for thoroughness of treatment, and, except for the last chapter, this book supports such an opinion.

Analytical Geometry of Conic Sections and Elementary Solid Figures. By A. B. GREEVE. Pp. xv + 314 + xiv. 9s. 1925. (Bell.)

This book claims to contain the substance of the plane and solid analytical geometry (except the straight line and circle) required for Pass and Engineering Students at Universities, and for the more advanced pupils of Secondary Schools. It is refreshing to find plane and solid geometry in one volume, but the plan possesses the inherent objection that not very much ground in either can be covered in one book. Furthermore, "any portions of the subject which are difficult or of algebraic interest only" have been avoided. The book therefore consists in the main of what may be called numerical analytical geometry of the conics and conicoids. There are also chapters on the General

Conic, Poles and Polars, Confocal Conics and Conicoids, and a final chapter on Curvature; but the great method of abridged notation, which is the very essence of the subject, is only hinted at in one chapter. The book is thus very limited in its outlook, and professes to be so.

An elementary knowledge of the Calculus is assumed; but alternative methods are given, usually in the examples at the end of a chapter. No mention is made, however, of Burnside's beautiful method for writing down secants. Perhaps the author regards this as artificial and "difficult." In any case it might have found a place among the examples, which on the whole are a very good collection. There is a short historical note at the beginning, and an admirable plate showing photographs of models of the conicoids. The book is beautifully printed and the diagrams are clear. There is one bad misprint on p. 157.

On the whole, this book is suitable for those reading by themselves for external Pass Examinations at the Universities, but is not suitable for those who can attend lectures or who are at school.

Higher Mathematics for Students of Engineering and Science.

By F. G. W. BROWN. Pp. xi+488. 10s. 1926. (Macmillan & Co.)

It is interesting for a "mere mathematician" to know what parts of his subject are required by students of Engineering and Science, and reference to the table of contents of this book supplies this knowledge. It is a diverse collection of scraps, including plane and spherical trigonometry, partial fractions, determinants, simple series, analytical geometry of two and three dimensions, and, of course, a considerable amount of calculus. "No attempt has been made to treat the bookwork with the utmost rigour required in modern mathematical theory," but "a list of books for reference has been given where complete and rigorous proofs may be found." This implies the pious aspiration that the reference books will be consulted; but it is doubtful whether an engineering or science student has time to do this.

Hoping to learn something from the engineer, the mere mathematician who is writing this review turned at once to the section on approximate methods, and found to his astonishment that no mention is made, either in the bookwork or examples, of Weddle's rule or of Dufton's rule. Turning again to the chapter on numerical and graphical solution of equations, Vieta's method is not mentioned, while the algebraic solutions of the cubic and quartic given might well have been omitted, since, though interesting, they are of no practical value.

On the whole, it is probable that this book contains too much theory. The author admits that a much fuller treatment is given in many cases that is demanded by the syllabuses for an Engineering degree, and hopes therefore that the book will be of service to those reading for degrees in Arts or Science as well as in Engineering. It would have been better to concentrate on the Engineer and Experimental Scientist. For this is not a book for those reading mathematics: it does not contain enough.

Apart from the general plan of the book, there is much to praise in its actual content. What is attempted is well carried out: the bookwork is clearly stated, and the examples are admirable. The volume is beautifully got up, and there is an index.

N. M. G.

Mathematical Theory of Finance. By T. M. PUTNAM, Ph.D. Pp. xi+135. 8s. 6d. net. 1925. (New York: John Wiley & Sons.)

This is a second and enlarged edition of an elementary book dealing algebraically with interest, annuities-certain, bonds, depreciation, and life assurance in its simplest forms. The book is useful and, allowing for the self-imposed limitations, it is to be recommended. Our adverse criticisms are that the book is expensive, and that, so far as English readers are concerned, the American terms are a drawback and the title exaggerates the contents. The definition of "annuity" is too restricted. There are good collections of examples for numerical work, and answers are given to some of them; it would be an improvement if the answers were printed at the end of the book and extended to cover all the questions. There are a few unimportant misprints.

W. PALIN ELDERTON.

Exercise Papers in Elementary Algebra. By the Rev. E. M. RADFORD. New and revised edition. Pp. viii + 112. 1s. 6d. 1926. (Dent.)

Mr. Radford's excellent set of papers has been revised and brought into line with modern practice. It consists of 100 papers covering the ground of the various Secondary School or Training College Examinations. Most of the questions are original, and practice in writing out bookwork is secured by an additional group of ten papers. The book is clearly printed, and in these days it is not out of place to draw attention to its price.

Test Papers in Algebra. By A. E. DONKIN. With Answers, 2s. 6d.; without Answers, 2s. Pp. 113. 1926. (Pitman.)

Mr. Donkin's useful *Test Papers in Arithmetic* were soon followed by a similar set of 100 papers, not as a rule crossing the limits shown in papers set in recent London Matriculation Examinations. There are plenty of exercises in graphs, logarithms, partial fractions, and of questions involving applications of functional notation, and the use of the remainder theorem.

A Condensed Collection of Thermodynamic Formulas. By P. W. BRIDGMAN. Pp. 34. 6s. 6d. net. 1925. (Oxford Univ. Press, for Harvard Univ. Press.)

These formulas apply to systems for which pressure and temperature are a possible set of independent variables, and for first derivatives of a system (e.g. water and steam in contact) in which pressure is a function of temperature, and temperature and volume are a possible set of independent variables. In pp. 24-34 the compiler explains the use of the Tables, and discusses the extension of the formulas to other systems.

(1) **Scholarship Arithmetic. Practice Tests.** Pp. 56. 8d. 1925. (Bell & Sons.)

(2) **Puzzle Papers in Arithmetic.** By F. C. BOON. Pp. 55. 1s. 6d. 1925. (Mills & Boon.)

(3) **Rapid Business Arithmetic.** By R. W. M. GIBBS. Pp. viii + 165. 2s. net. (Oxford Univ. Press.)

(4) **Elementary Mathematics. Part I.** By E. SANKEY and A. ROYDS. Part I. (for pupils of 11-13 years). Pp. 117. 1s. 6d. 1926. (Bell & Sons.)

(1) Those who are called upon to prepare or to examine candidates for free places, scholarships to secondary schools, or promotion to central schools will find this *brochure* very useful. Apart from the Group Practice Tests, there are sample papers set by some forty different Education Authorities. Such a collection as this is, we believe, the only one of its kind.

(2) "Tests of common-sense and ingenuity"; combining "the essential features of the Magazine Problem and the Intelligence Test"; set as a weekly Puzzle: "designed to encourage experiment, systematic tabulation, inductive conclusion, and a wise use of trial and error." In these phrases the compiler fairly characterises most of an excellent set of well-produced questions. There are plenty of "missing digit" problems, card tricks, elementary number theorems and the like, and the whole is an amusing and well-thought-out collection. The sale would, no doubt, be increased if laymen with an interest in such puzzles, and with but slight mathematical equipment, knew where to get the answers. Not the least amusing feature in the collection is the number of ways in which Mr. Boon contrives to enforce his adage—Good Explanation is the chief part of your Answer.

(3) "The aim of this book is to bridge the gulf between the Arithmetic of the Schoolroom and that of the Business House." Walking as delicately as Agag, the youth "in the latter years of a school course" has his course marked out for him by such stepping-stones as: "To divide one fraction by another, turn the divisor upside down and multiply": "in short division of a number containing decimals, by a whole number, the point in the quotient comes under the point in the number that is being divided": "to divide a number of shillings, pence, and farthings by 2 (to the nearest farthing above), first take half of the shillings; and then half of the pence and farthings (to the

next halfpenny above)." The first forty pages consist of rule-of-thumb instructions of this type. That this is the last resource of the teacher would be a damning criticism of Arithmetic as taught "up to the latter years of the school course."

On the other hand, if such a preparation for the business world is necessary, Mr. Gibbs has done his work well. The second half of the book, with its instructions in the use of Tables and the Slide Rule, is carefully thought out and provided with plenty of appropriate Exercises.

(4) It is not inspiring to open this book at random and to be confronted with $\frac{1}{16}$ converted to a decimal by means of a full-dress long division sum. The writer now feels that he should not have been as surprised as he was when he recently found similar performances with divisors of 3000, 9000, etc., etc., in which the examinee proved faithful to his trust and not a single "nought" was missing from the long array.

The book is "specially prepared for Central Schools, Senior Elementary Schools, and Upper Standards (VI, VII, VIII). It revises and extends the arithmetic which children up to 13 take in the elementary schools and covers the elements of algebra as far as simple equations and the "first four rules," with simple practical geometry of the line, triangle and circle, and the usual introduction to mensuration. It is clearly printed, and contains an ample selection of exercises. It should prove adequate for the purpose intended.

A Practical Treatise on Fourier's Theorem and Harmonic Analysis for Physicists and Engineers. By A. EAGLE. Pp. xiv + 178. 9s. net.

Mr. Eagle has taken Fourier's Theorem to apply solely to periodic functions. His "artificial functions" are functions arbitrarily defined as "equal to one analytic function in one part of their range and equal to another one in some other part." To empirical functions, derived from graphs, etc., of experiments and observations, and to artificial functions he applies Fourier's Theorem and the method of Harmonic Analysis. The approximations he finds for the coefficients are in several cases very neatly obtained, and his "new method of Harmonic Analysis," consisting in joining the tops of the given ordinates by straight lines or by arcs of parabolas of the second or third degree, certainly has the advantages he claims for it. The author has his eye on "some modern writers on pure mathematics" in venturing on the term "artificial function"; he cannot have had them in view when he included parts of pp. 1-11, which might be dispensed with. Apart from this blemish, we congratulate the author on doing an ingenious, carefully thought out, and very useful piece of work.

The Mechanical Investigations of Leonardo da Vinci. By I. B. HART. Pp. vii + 240. 4s. 1925. (Open Court Co.)

Dr. Hart is a Research Assistant in the Department of the History and Method of Science at University College. He is an A.F.R.Ae.Soc., and two chapters of this volume have appeared in the *Journal of the Society*: other sections of the book have been read or published elsewhere. The author's first intention was to study Leonardo's work on aeronautics alone, but he wisely widened his scope, and has dealt with the mechanical science of the fifteenth century and the influences contributory to development of the subject in the hands of the amazing Italian. He has given an admirably clear account, which is all the more useful to the average student because French and German sources alone have been hitherto available.

The sections dealing with Leonardo's pioneer work—work in which he owed little or nothing to anything but his own genius—are of extraordinary interest. The passages from Pettigrew and Maxim compared with corresponding extracts from the Italian manuscripts, and the descriptions of the actual attempts at the construction of flying machines are alone enough to reveal the really scientific character of Leonardo's work, and to the fact that he had grasped 400 years ago the principle of the parachute and the helicopter, had the idea of "lighter than air," of stream-lines, and even of shock-absorbers. Dr. Richter's collection of Extracts from the Note-books appeared in the late eighties. An article in the *Nineteenth Century* for July 1910, on "Leonardo da Vinci and the Science of Flight," by Mr. E. M'Murphy, followed a volume

by the same author, entitled *Leonardo da Vinci's Note-books. Arranged and rendered into English with Introductions*. The latter contains some 15 pp. of passages on flight translated from various MSS., and a more detailed account of the work of Leonardo as forerunner is given in the former. This is all, as far as we know, that has been attempted in the way of a translation of the MSS. Dr. Hart claims that his translation of the MSS. *On the Flight of Birds* is the only translation of any complete manuscript by Leonardo in English, and that the sections on aeronautical work form the most complete account of its kind. There are one or two points which require attention as having escaped the proof-reader. On p. 148, ll. 1-2, is a meaningless passage which should run something like this: "We are able from these observations to deduce that the most rudimentary of these movements can be grasped by man's understanding." On p. 169, l. 13, for "winds" read "wings." P. 174, l. 5, after "above the wind" requires "and then turn and face it."

Historic Instruments for the Advancement of Science. By R. T. GUNTHER. Pp. ii + 90. 2s. 6d. net. 1925. **Old Ashmolean Postcards.** Mathematical Series. Asst. of 8 for 6d. (Oxford Univ. Press.)

This is "a Handbook to the Oxford Collections prepared for the opening of the Lewis Evans Collection on May 5, 1925," and a delightful little companion it is, having not only the charm of the guide who is brimful of a subject with which he is completely familiar, but the advantage of enabling us in after days to recall anything that we care to revise in the remarkable variety of interesting information in its ninety pages.

Perhaps most notable of all the remarkable things in the collection is the series of Astrolabes, sixty-three in number, varying in dates from A.D. 984 (the earliest scientific instrument known) to that made of paper by Prujean in Oxford, c. 1670. This historical introduction to the section treating of these instruments should send many readers to catch across five centuries something of the pathos in Chaucer's instructions to his ten-year old "litell Lewis my sone," in the use of the "smal instrument portatif aboute," a "suffisaunt Astrolabie as for oure horizonte, compowned after the latitude of Oxenford." Seventeen pages are given to descriptions of the series of portable and other dials.

The mathematical instruments include Napier's Bones, Morland's Adding Machine, Slide Rules, and Oughtred's Circles of Proportion. An appeal for even models of the Cross-staff, Back-staff, and Cross-bow, without which the collections of surveying instruments is incomplete, will, it is to be hoped, meet with success. A recent discovery in the Library of St. John's has revealed Humphrey Cole, 1586, as the first known maker of the Theodolite. We have alluded to but a tithe of the matter to be found in these pages.

One set of the *Old Ashmolean Postcards* is called a Mathematical Series. Five of these give Albert Dürer's Alphabet, illustrating his application of Geometry to Art, 1525. The rest are from the works of Robert Recorde, Fellow of All Souls, and comprise reproductions of: a passage from the Ground of Arts, 1542, on the sign of multiplication, and another from the Whetstone of Witte, 1557, on the sign of equality; title page of the Pathway of Knowledge, 1551, the second Geometry in English; and title page to the Whetstone of Witte, the first Algebra in English. All are beautifully reproduced.

The History of Mathematics in Europe. By J. W. N. SULLIVAN. Pp. 109. 2s. 6d. net. 1925. (Oxford Univ. Press.)

This monograph is Chapter IV. in the *Chapters in the History of Science*, of which Dr. Charles Singer is the general editor. Its scope is defined in the sub-title: "From the Fall of Greek Science to the Rise of the Conception of Mathematical Rigour."

Mr. Sullivan has made for himself a name by his powers of lucid exposition, and in the present instance he has not belied his reputation. The result is an attractively written summary of the development of the subject, which should be on the shelves of every secondary or higher school in which such elementary works of reference are in demand. It is also a work in many

respects of inspiration, and should send many readers to the volumes by Ball, Cajori, and, later on, to Cantor, to examine for themselves much on which exigencies of space and other considerations have limited the author to little more than a passing reference.

The illustrations were selected by Dr. Singer. The value of the monograph as a work of reference, especially for young people, is greatly impaired by the lack of an Index. The author should, for the purposes of separate publication, have remembered that to most of his readers this would not be a "chapter."

Mélanges de Mathématiques et de Physique. By E. PICARD. Pp. viii + 366. 20 fr. 1924. (Gauthier-Villars.)

This is a companion volume to the *Discours et Mélanges* which appeared in 1922, and contains the overflow of historical and philosophical material which found no place in the volume, together with other papers of a general character dealing with mathematical physics. The biographical essays deal with Weierstrass, Sylvester, Hermite, Abel, Stieltjes, Zeuthen, Pascal, and Fizeau. Reference to the migrations of Sylvester before he went to Oxford evoke allusions to the career of his contemporary Cayley and his one-time divided allegiance between law and mathematics. "Here we see nothing of the almost monotonous uniformity of the career of a French mathematician. I do not go so far as to suggest that French mathematicians should begin at the bar, but it is a pity that some of the most distinguished among them did not in the early stages of their career imitate the German mathematician and go abroad for a while." Among the personal reminiscences is one which will raise a smile in those who remember the restless and impetuous nature of Sylvester. As M. Picard exclaims: "Quel contraste entre le génie si pondéré et si sage de Cayley et l'imagination créatrice toujours inquiète de Sylvester!" During a visit to Paris, Sylvester asked Picard if he could master the theory of Elliptic Functions in six weeks, and if so, would he recommend to him a competent mathematician to run through the subject with him in two or three lessons a week. The man was found, but, before the second lesson was over, reciprocants and matrices triumphed over elliptic functions, and, perhaps not unwillingly, the teacher became the taught and was initiated into Sylvester's latest researches. But there were no more lessons. M. Picard continues: "He was an artist and an enthusiast. Once he was struck by the beauty of a problem he pursued its solution remorselessly, often incurring great waste of time. He had none of that serenity in the choice of subjects which so often prevents one's labours from being premature and fruitless." He was also a poet. "In his last visit to Paris, in the autumn of 1895, I remember a breakfast with one of our colleagues, at which he recited an elegy he had just composed in Latin verse. One of us said that it reminded him of Tibullus, a remark which moved Sylvester to tears. . . . The memory of the illustrious mathematician, of the man so warm-hearted and enthusiastic, so kind and lovable, will ever remain with those who had the honour to know him."

The lecture on Hermite occupies 30 pp., and is a careful study of his work and influence. Space must be given to a few quotations: "To read his great Memoirs on the Algebraical Theory of Binary Forms leaves on the mind an impression of simplicity and strength; no mathematician of the nineteenth century more closely possessed the secret of those profound and recondite algebraical transformations which, once they are discovered, appear so simple. It was to such an art of algebraical calculation as this that Lagrange no doubt referred when he observed to Lavoisier that Chemistry would some day be as easy as Algebra." "About 1855, Hermite was in close correspondence, first with Jacobi, and later with Dirichlet—who at the moment was perhaps the more likely fully to grasp the significance of his researches. From the beginning he received from them the kindest welcome—a welcome he never forgot. Only a few weeks before his death he wrote that he had always been and would be to his dying day the disciple of Gauss, Jacobi, and Dirichlet. Affinities between minds of the first order are always interesting and useful to follow." Of Hermite as teacher we read: "Never to be forgotten by those who heard him is the memory of his incomparable teaching. What marvellous

causeries he gave us, the gravity of his voice transformed at times by enthusiasm when even the most elementary question would seem in a moment to bring to our view vast horizons on which we saw the Science of the future taking its place beside the Science of to-day. Never was teacher less didactic or more stimulating. The only man in all my experience to whom I can compare him is Wurtz. To both, in different forms, the work of teaching was as solemn and inspiring as the message of an Apostle: I have known men in their audiences, with but little scientific equipment and out of place in the lecture-rooms of the great mathematician or the illustrious chemist, simply dazed at the discovery that a lesson in Chemistry or in Analysis could be so poignant and dramatic. Hermite reminded one of Pasteur, and he would have made his own the phrase which was so often on the lips of his great contemporary: 'La Science se fait non seulement avec l'esprit, mais aussi avec le cœur.' The purely professional aspect of the addresses in this volume is momentarily illumined by many such reflections and memories as the above.

BRITISH ASSOCIATION.

OXFORD, AUGUST 4-11.

MATHEMATICS has not recently been important among the activities of the British Association, but an attempt is being made this year to provide a programme of sustained interest. In Section A we are associated with astronomers and physicists, and although their discussions sometimes entertain us we have never been confident of our ability to entertain them. At Oxford on three mornings the section will divide, and several speakers have undertaken to describe recent lines of advance and outstanding problems that are being attacked, in terms intelligible to mathematicians who are not themselves experts in the particular branches of mathematics concerned.

A complete subsectional programme is not yet available, but we learn that Prof. Burkill, Mr. Francis, Prof. Hardy, and Mr. Titchmarsh are expected to deal with integration and trigonometrical series, Dr. Cherry with dynamical equations, Prof. E. A. Milne with a problem in radiation, Mr. Ramsey with mathematical logic, Mr. Newman with n -dimensional topology, and Mr. Chaundy with commutative operators. Prof. Dixon and Prof. Elliot have promised their support, and among visitors whose co-operation is assured are Prof. Archibald, Prof. Bjerknes, and Prof. Carathéodory.

It rests with the mathematicians of the country, amateur as well as professional, from the schools as well as from the universities, to prove by their attendance that an effort to give in simple language some idea of the present vitality of mathematics appeals to a genuine interest. Only a fraction of the possible subjects can be dealt with at one meeting, and if this year's experiment is successful, the British Association will have discovered a function which can be continued beyond the circle surrounding its present origin and which may even be found to be regular everywhere.

380. When Descartes was asked whether the clattering of wooden shoes in the streets of Amsterdam did not disturb his meditations, he said, "No more than would the babble of a rivulet."

381. Modern art looks like the Second Book of Euclid.—Sir Chas. Biron.

382. Blasius was one of the earliest (circa 1513) to use "substractio," as the Dutch and English did for some generations.—Prof. D. E. Smith.

383. Her favourite science was the mathematical.—*Don Juan*, Canto i. 12.

384. Maupertuis of folly's as prolific

As Newton is of theories scientific.

—*La Pucelle*, Canto 17 (Corisandre variant).

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From the Rev. **W. H. Lavery**, his pamphlet on curve tracing and his introduction to dynamics:

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<i>The Laws of Motion</i> - - - - -	1889

Also offprints of papers by J. Griffiths and A. J. Pressland, valuable runs of the *Gazette* and of the *Proceedings of the L.M.S.*, and interesting material, such as membership rolls, from the early days of the A.I.G.T.

The Rev. W. H. Lavery is one of two members, the other being the Rev. E. F. M. MacCarthy, whose names were on the preliminary A.R.G.T. (*Reform*, not *Improvement*) list dated 1870, and whose membership has been continuous since then. Canon Wilson's name was on that list, but he had ceased to be a member when the occasion of the jubilee recalled him to the Association. Mr. A. A. Bourne and Sir Thomas Muir joined the A.I.G.T. on its foundation, but were not among the actual founders.

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